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VOL. 5 JANUARY-FEBRUARY, 1915 No. 1

THE WHITE RAT AND THE MAZE PROBLEM

I. THE INTRODUCTION OF A VISUAL CONTROL

STELLA B. VINCENT

From the Psychological Laboratory of the University of Chicago

WITH EIGHT FIGURES

Small[1] was the first to emphasize the importance of the kinaesthetic sensations in the life of the rat, while Professor Watson,[2] by eliminating the senses one by one, showed that in learning the maze, at least, senses other than the kinaesthetic and tactual can easily be dispensed with. The natural conclusion was drawn that in such problems these animals use vision, hearing, olfaction, etc., but slightly if at all.

The recognition of the functional value of the kinaesthetic sensations and the dominant part which they play in such a bit of learning as this has been of the greatest possible value. The question is immediately raised, however, of what intrinsic value are eyes, ears, etc., if not for learning. Or, the question might be put in this way: Is this motor co-ordination and the mode of learning entirely different from those other habits which the rat acquires naturally in its usual environment.

Professor Watson himself anticipated further work when he says, " We have supported everywhere the negative conclusions of Small. We, no more than he, offer positive evidence that the kinaesthetic are the only necessary factors in the maze association. Both of us alike used the method of elimination . . . We feel that we are now in a position to begin the study of the positive aspects of the problems offered by the behavior of the rat in forming the maze association."[3]

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[1] SMALL, W. S. Development of the Young White Rat. *Am. Jour. Psychol.*, vol. 11, p. 234. 1899.

[2] WATSON, J. B. Kinaesthetic and Organic Sensations. *Psychol. Rev. Mon. Sup.*, vol. 8, no. 2. 1907.

It was this positive aspect of the situation which formed the basis for this study. Our contention was that other senses might enter into the learning of such a problem and modify it. We believed that the true path and the false in the maze might be made to differ so in brightness, in olfactory qualities, in tactual values, etc., as to affect the establishment of the habit. Our object was to find out if the learning process was thus affected and in what particular ways. The first experiments were concerned with an attempt to introduce sight as a control in the formation of this habit. The maze as ordinarily constructed and as used by Professor Watson is all of wood and of one color and is not favorable for the use of vision. The sides of the runways are high enough to prevent any visual help from outside unless from above. It has been shown many times that human subjects under such conditions find it difficult to obtain or to use visual clues. Ours was not the first attempt to introduce visual control in the maze. Others had tried the same thing in different ways but the stimuli which they used were ineffective and the investigators did not carry their experiments far enough to make any final statements. Although so much has been done with rats the proof of their ability to use vision in any exact way was not very conclusive.

PREVIOUS WORK

The previous lines of experimental evidence as to the effectiveness of vision are several: first, the comparison of the time of learning and speed in running of normal rats trained and tested in the dark and light respectively; second, a comparison in the same respects of normal and blind animals.

Professor Watson based his conclusions, as to the uselessness of vision in the maze, upon the fact that normal rats trained in the light could run the maze as quickly in the dark; that normal rats could learn the maze in the dark and acquire as rapid speed as in the light; and that blind rats could learn the maze and run it with a speed equal to that of their normal companions. It has been shown by others that it is scarcely fair, in such a situation, to make speed the sole criterion of the learning process.

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[3] Ibid, p. 96.

It must also be remembered that many experimenters who have attacked this problem fail to separate in their detailed results and conclusions the typical learning process, which is shown chiefly in the first few trials, and the perfecting of the automatism, the acquisition of speed, which follows. Possibly Professor Watson's work may bear this criticism.

The third line of evidence comes from attempts to introduce visual clues at critical points in the maze. Small fixed colored posts at such places and also varied the direction of the light which fell upon the maze. He concluded that the use of vision was not shown by any discrimination or recognition.[4] Miss Allen marked the path for another rodent, the guinea pig, with colored cards but her results were negative.[5] Professor Watson, in the case of one rat, used colored lights with no perceptible effect.[6] All of these objects were stationary. It is possible that moving objects might have been better. Rats are hypermetropic and such animals may well fail to respond to near objects in any discriminating way.

The fourth form of attack upon the visual powers of these animals consists in observations of the animals and experiments with them in the dark and in the light respectively and noting the amount of activity and the accuracy of movement. Opinions differ slightly here. Slonaker shows in his studies of the activity of the white rat that its greatest period is during the night, beginning with the first shadows of afternoon. He says that the average distance traveled is five miles per night as against one-tenth of a mile per day and concludes that light normally does have an influence upon the animal's activity.[7] A few of Yerkes' dancers seemed somewhat disturbed by tests in the darkness.[8] Miss Allen found that her guinea pigs made more random movements in the darkness than in the light.[9] So far as I know no one has followed this lead further. It would be an interesting bit of experimental work.

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[4] Op. cit.

[5] ALLEN, JESSIE. Association in the Guinea Pig. *Jour. Comp. Neur. and Psychol.*, vol. 14. 1904.

[6] Op. cit., p. 43.

[7] SLONAKER, J. P. The Normal Activity of the White Rat at Different Ages. *Jour. Comp. Neur. and Psychol.*, vol. 17, p. 342. 1907.

[8] YERKES, R. M. The Dancing Mouse. P. 189. 1907.

[9] Op. cit.

The fifth line of proof lies in the comparison of blind and normal animals as to their accuracy of movement. In the experiments reported in Orientation in the Maze where, by means of a removable section, the maze could be lengthened and shortened there was a blind animal which had trouble with turns which the normal rats made correctly. The authors say of some other (normal) animals, "Since two out of the eight animals made eight out of the nine unquestioned immediate orientations we are willing to admit the possibility of the use of distance sense data in their cases."[10] Miss Richardson in some jumping tests with rats, where both direction and distance of the jump were varied independently of each other and also varied from habitually established norms, concluded that the visual stimulus furnished a control as to the direction of the jump but failed to afford any accommodation to changes in distance. Concerning some tests with problem boxes, she says they afforded no conclusive evidence as to the functioning of visual impulses. "The lack of vision, however, was disadvantageous in proportion as the problem demanded finely coordinated and narrowly localized movements."[11]

The sixth method of investigation is more directly concerned with the sense itself. The Watsons concluded from experiments with spectral lights that there is good if not conclusive evidence, since green was not used, that the responses were made to differences in intensity and not quality. Similarly Miss Weidensall showed in a critique of discrimination that in experiments with black and white the white was twice as effective as the black and that in most discrimination experiments with two objects only one of the objects may have any regulative control, the other being neglected.

The use of sight by some animals, as birds and monkeys, is admitted in connection with such problems but we have confined this report to rodents whose vision is of a common type.

The following views are held as to the place of vision in the labyrinth problem: (a) The control is kinaesthetic *par excellence*, coupled probably with tactual and static and possibly with organic sensations; (b) Vision may have a tonic stimulating effect or it may serve merely for general orientation; (c) Vision is not for perceptual purposes—"The rat does not hang his associations upon gross and obvious (visual) objects;" (d) Sight may lessen random movements; (e) It may give general direction if not accurate distance; (f) Vision may even be a hindrance in such problems.

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[10] CARR, HARVEY, and WATSON, J. B. Orientation in the White Rat. *Jour. Comp. Neur. and Psychol.*, vol. 18, p. 27. 1908.

[11] RICHARDSON, FLORENCE. A Study of the Sensory Control in the Rat. *Psychol. Rev. Mon. Sup.*, vol. 12. 1910.

In a previous paper[12] facts have been given which show that the rat's vision is weak. It may be possible that a rat does not discriminate stationary objects and it is probable that brightness is the most effective factor. In this work, however, there was no attempt made to substantiate such opinions or to evaluate the visual sense. The only interest lay in the attempt to see whether vision could not be introduced into the labyrinth problem as a control and if this could be done then to determine what was its effect upon the establishment of the automatism.

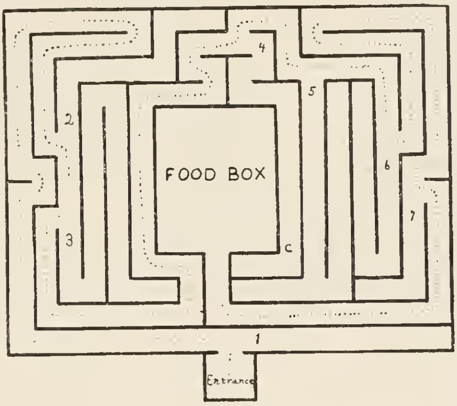

FIG. 1. Plan of maze

APPARATUS AND MODE OF EXPERIMENTATION

The maze used was the modified Hampton Court Maze which Professor Watson describes in "Kinaesthetic and Organic Sensations." [13] Indeed it was one of the same mazes which he used in his experiments. The only change in the construction was the blocking of pathway "C" at the farther end to make it a blind alley instead of a longer way around (Fig. 1). The essential difference, however, was the fact that the true pathways and the blind alleys were made to differ as far as possible in brightness. Black cardboard lined and covered the one and a very white paper lined the other. In the later experiments black and white enamel paint was used on the floor and sides while black cardboard covered the top of the black pathway.

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[12] VINCENT, S. B. The Mammalian Eye. *Jour. of Animal Behavior*, vol. 2, no. 4, pp. 249-255. 1912.

[13] For detailed description see "Kinaesthetic and Organic Sensations," op. cit., p. 16-18.

The problem box for the discrimination tests was a pasteboard box 12′ x 12′ x 9′, with round tubes inserted at the floor level in opposite sides. One tube was white, with white oiled paper pasted in the upper third to increase the brightness, the other tube was black. The tubes were not straight but were bent in the middle at right angles to prevent any entering light from the end of the open tube from giving a clue. Only the tube of the brightness for which the animal was being trained remained open at the end. The box was turned in an irregular order to prevent choice by position.

The only stimulus to the activity was the reward of food at the completion of a successful run in the maze or the choice of the right exit in the problem box. Five animals usually constituted a group.

Some records were first made upon the maze as it originally stood with walls and floor of unpainted pine. A group of untrained rats was given 50 trials covering a period of 18 days. These rats were then taken over to the problem box where they were given 50 trials. Then another group of rats which had previously been given 50 trials on the problem box was put in the maze for 50 trials. Ten trials a day were given in the problem box, but on the maze only 3. These records are referred to as the normal records and furnish the standard for comparison.

The maze was then made black and white, the true path black (see dotted line, Fig. 1) and the blind alleys white. Two other groups of rats learned both it and the problem box in alternation as above. This maze is sometimes spoken of as the black maze.

The third change consisted in making the true path white and the *cul de sacs* black and using two new groups of animals in the manner described above. This maze is occasionally referred to as the white maze.

In brief the experiments upon which this discussion is based are as follows:

1. Normal maze records—(a) rats trained; (b) rats untrained; (c) discrimination experiment for (a); (d) discrimination experiment for (b).
2. Black-white maze records, true path black—(a) rats trained; (b) rats untrained; (c) discrimination experiment for (a); (d) discrimination experiment for (b).
3. Black-white maze records, true path white—(a) rats trained; (b) rats untrained; (c) discrimination experiment for (a); (d) discrimination experiment for (b).

The reason for the use of the two pieces of apparatus was this: After the first group of animals had learned the black and white maze we wished to see whether it was really brightness to which the animals were reacting. The other experiment —the box with the black and white exits—was devised, therefore, to see whether the brightness experience carried over. Then the suggestion arose that the differences which were seen in the conduct of the rats might not be due to brightness alone but that some of the changed results might be a general effect of training. To meet this criticism not only were the experiments doubled by the use of both maze and box, but groups of animals trained upon the box afterward learned the maze and animals trained upon the maze learned the box. In order to make the normal group comparable the training also had to be included in their case although the maze was uniform in brightness.

It may be objected that the contact values of the two media, paper and cardboard, used in the first experiments differed and that the air pressure, sound qualities, etc., were not the same in the two paths. The work will have to face that criticism. The training tests upon the problem box and the succeeding experiments upon it seemed to show, however, that it was really brightness to which the animals were reacting. The six blind animals used upon the black-white maze furnished a further control. If there were other sensory elements fused with vision it does not affect the value of the experiment since this is normally true and since it was the visual element which was the variable one.

The tables submitted show only the time of a total reaction and the errors. Leaving the true path, entering a *cul de sac*, was counted as one error. Returns could not be counted as errors since part of the maze was covered. For the same reason we can say nothing about the total distance traversed. All of the tables upon which this discussion is based would gladly be given but it is impossible within the limits of this paper. The results will be shown by means of graphs upon which the discussion will be based and only such other additional data will be given as is necessary.

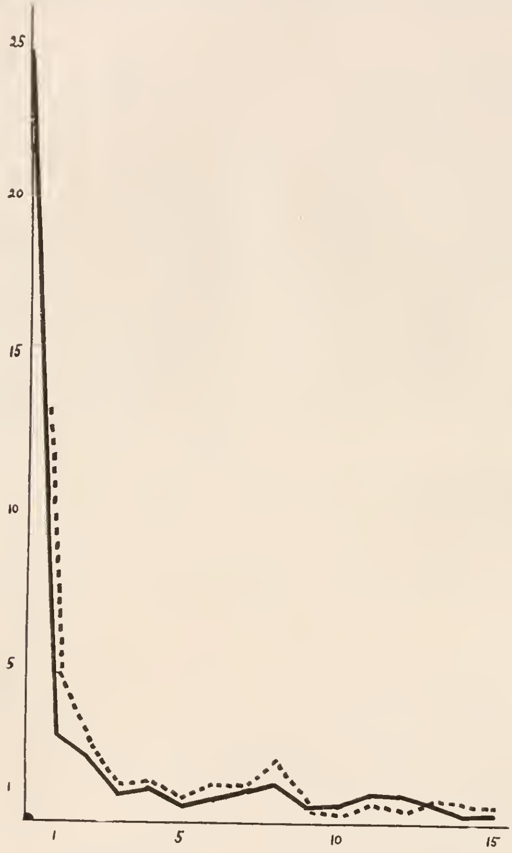

FIG. 2. Learning curves of 10 rats on the normal maze
———— Time, - - - - - - - - Errors

Four months' work was put upon this problem in 1907. Nothing more was attempted until it was resumed in 1910. None of the first experimentation is reported here, since it was all repeated in the later series under stricter control and with similar results.

NORMAL MAZE

There is no need in this place of giving a long description of the general behavior or giving the individual details of the experimentation with the groups of rats in the normal maze. The conduct differed in no respect from that noted by so many others. The curve of learning may be seen in Fig. 2. The description of the mode of plotting this curve may be found in my monograph on the tactile hair.[14] The actual time and the number of errors for the first 10 trials are given in Table 1. These records are made from the combined records of two groups of animals, one of which had been previously trained upon a black-white discrimination box. As this training proved to have so little effect upon the subsequent maze record these pages will not be burdened with the numerical results.

TABLE 1

TIME AND ERROR RECORDS, FIRST TEN TRIALS, ON NORMAL AND BLACK-WHITE MAZES

	Time		Errors	
Trial	Normal Maze	Black-white Maze	Normal Maze	Black-white Maze
1	1804 sec.	1342 sec.	14.9	7.5
2	966	413	11.9	4.3
3	542	254	10.4	3.
4	847	211	7.4	3.
5	233	98	4.1	1.6
6	193	72	3.5	1.
7	63	37	1.6	.5
8	49	48	1.4	.2
9	37	54	1.5	.5
10	33	39	1.1	.4

The training upon the box, on the whole, seemed slightly disadvantageous to the group of animals which later learned the original maze. There was not so high a degree of accuracy as evidenced by the number of errors and the average time taken per trial was longer than that of the other group which had had no training. To account for this is not difficult when we consider that the problems are distinctly different. In the one case there is an immediate reaction, in time too brief to be taken, to a situation which offers but two alternatives and in which brightness is the determining factor. In the other case there is a devious way to learn which involves many turns and the possibility of many false turns. The final escape to food takes a time which varies from two hours per trial at the beginning to ten seconds when the problem is learned. The habits set up by the brightness contrast, whether depending upon discrimination or not, clearly cannot be carried over advantageously to a situation where the contrast does not exist. It may easily be conceived also that the motor habits involved in the simpler reactions described above for the problem box which bring the rat immediately to the presence of its food might be disastrous and delay the acquisition of a reaction depending upon the co-ordination of a long series of acts and extending over a considerable period of time.

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[14] VINCENT, S. B. The Function of the Vibrissae in the Behavior of the White Rat. *Behavior Mon.*, vol. 1, no. 5, p. 15. 1912.

As the learning of this maze has been so fully and freely discussed before all mention of it will be neglected here and any facts of interest concerning it will be brought out in the comparison of the two mazes which will follow later.

BLACK AND WHITE MAZE

The outcome of the tests on the black and white maze was noticeably unlike that of the normal maze. The differences were seen in the behavior of the animals and appear in the numerical results and the graphs plotted from them. They were confirmed and checked by the data furnished by the blind animals and by the facts brought out in the box experiments.

Success in such a problem has several measures: (a) the time taken relative to the total distance, i.e., speed; (b) the number and distribution of the errors, i.e., accuracy; (c) the time of learning, i.e., the number of trials in learning; (d) the surplus values of time and errors; (e) the rate of elimination, i.e., distribution of effort; (f) the form of the learning curve—a picture which reveals some of the complex relationships existing among the various factors. In this paper the burden of proof will be put upon accuracy and speed, since it was in these two respects that the greatest divergence was seen. The other criteria, however, will not be entirely neglected.

In the interest of clearness as well as of time and space, the results from the four groups of animals, trained and untrained, for the black and for the white maze, will be combined and presented as a whole. The differences were unessential. Any evidence of training being carried over from the box to the black-white maze was so slight that it may be neglected. This is due, doubtless, to the fact that the two problems were so dissimilar and that the time on the box was so brief. On the contrary, there was decided evidence of the effect of training in animals which went from the black-white maze to the box but that will be mentioned in another connection. The results also, when the true path was white and when it was black, agreed entirely in the essential details and hence they may be massed. The minor differences will be used only by way of explanation or illustration.

SPEED

Speed is time as measured by the distance traversed. In this case it is impossible to state the total distance since the returns, in the part of the maze which was covered cannot be counted. However, some evidence can be offered. One of the first lines of proof is the observed conduct of the animals.

The behavior in the black-white maze was very unlike that in the normal maze. In the beginning trials there was less activity, more sluggishness of movement, fewer errors, yet slow runs. Time and again I find in my notes such expressions as: " Slow start." "A very slow walk around although without error." " Very little rapid running." " Slow movement compared with normal maze." " No running and little sniffing at food-box or anywhere else." " Little running but much actual sitting still." " Few errors but little running." " Do not seem at all frightened or curious." " Do not apparently use covered ways as places of refuge." " Errors do not seem attractive and there is no blind running which would make the animals blunder into errors." There was more hesitation seen in these rats. Even long after the problem was learned they slowed up or wavered in their running when they came to a *cul de sac.* Some typical records, where every move which could be seen was entered with the time which it took, may serve to indicate the lesser activity better than a general description.

First Two Records of Rat "1"

May 10th and 11th:
  1st trial:
    To first corner and back—10 sec.
    About entrance 55 sec.
    To first corner 10 sec.
    Here 2 min. 30 sec.
    Home—remains 9 min. 30 sec.
    First corner 15 sec.

Second corner 30 sec.
Reaches error "2" in 15 sec.*
Reaches error "3" in 5 sec.
Stays here 1 min. 15 sec.
Reaches food box 15 sec.
Reaches error "4" 15 sec.
Reaches error "4" 15 sec.
Reaches error "6" 11 sec.
Reaches error "7" 20 sec.
Back to "6" 1 min.
Back to "4" 10 sec.
Back to food box 10 sec.
Returns to "4" 30 sec.
Returns to "6" 20 sec.
On to "7" 1 min. 45 sec.
Back to "6", here 5 min. 15 sec.
Back to "4" 30 sec.
In "4" and out. Sits here 30 sec.
On to "6" 15 sec.
On to "7" 1 min.
Sits at "7" 3 min. 30 sec.
In "7" and out 30 sec.
Sits here 7 min.
On to food box 15 sec.
Total time 40 min. 22.6 sec.
Errors 2.

2nd trial, rat "1":
Slow start.
Reaches 2nd corner 5 sec.
Reaches error "2" 10 sec.
In and out error "2" 20 sec.
In and out error "3" 10 sec.
Reaches food box 30 sec.
Reaches error "4" 15 sec.
In and out "4" 30 sec.
Reaches "6" 5 sec. Here 20 sec.
On a short way 30 sec.
Returns to "6". Here 15 sec.
Back to "3" 1 min. 30 sec.
Returns to food box and back to "3" 1 min.
To food box. Here 3 min. 30 sec.
Back to "3". Here 30 sec.
To food box 1 min.
To "4" 15 sec.
Reaches "6" 15 sec. Here 15 sec.
In and out "6" 30 sec.
In alley near 1 min. 45 sec.
Back to "6". Here 45 sec.
Back to "4", to food box, to "3", 30 sec.
Back to "2". Here 1 min.
Back to food box 30 sec. Here 9 min. 30 sec.
On to "4", back to food box 30 sec.
On to "4", back to food box 15 sec.
On to "4", in and out 1 min.
On to "6" 15 sec.
On to "7" 45 sec.
On to food box 15 sec.
Total time 29 min.
Errors 4.

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* "2", "3", etc., are the numbers of the blind alleys in the Maze. See Fig. 1.

These individual records show the type of behavior described above, which clearly is not like that seen in the normal maze in the first and second trials. There, the rats cover three or four times the distance and make twice the number of errors. Here, the total distance traversed in the true path in trial one was 93 ft., making the rate about 2 ft. per minute. It is true that the rats were not moving all of the time but neither were the rats in the normal maze although they are far more active. The total distance covered in the true path in the second trial was 135 ft. The rate, therefore, was but little less than 5 ft. per minute. If the activity had been evenly distributed over the maze, this rat, in its first trial, should have been in the false path 18 minutes and in its second trial 13 minutes.

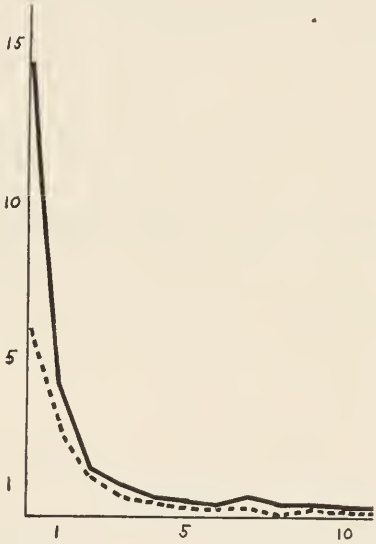

FIG. 3. Learning curves of 20 rats on the black-white maze
——— Time, - - - - - - - - Errors

From the figures of the rats in the normal maze I took those of the rat whose time and error records most nearly approached the average and computed the total distance, etc., for the first and second trials in the same way as above. This rat's time was a little low, so the speed is probably a little high for the first trial. The first trial showed a total distance covered of 313 ft. of which 190 ft. was in the true path and 123 ft. was in the blind alleys. The average speed for the entire distance was a little over 15 ft. per minute. The total distance for the second trial was 184 ft. of which 106 ft. was in the true path and 78 ft. in the *cul de sacs.* The speed then was 27 ft. per minute. Notice the distribution of the activity in this maze and compare it with that of the black-white maze above.

This slowness might be made more evident by another illustration. The average time of the first trial made without error for the rats on the normal maze was 45 sec. The average time of the 20 rats on the black-white maze for the same errorless trip was 122 sec. It took the latter group over four times as long. Returns were not counted in either case but so far as I can tell they were very few and fairly comparable.

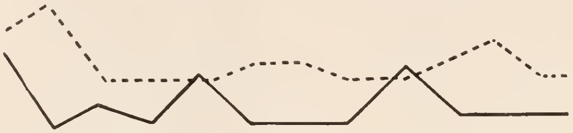

FIG. 4. Time curves for black-white and normal mazes, last 10 trials
——————— Normal, - - - - - - - - Black-white

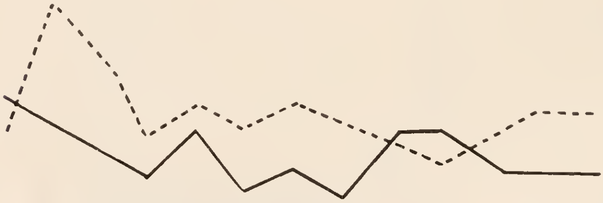

FIG. 5. Error curve for black-white and normal mazes, last 10 trials
——————— Normal, - - - - - - - - Black-white

But besides these individual records there are the combined group averages. The numerical data for the first ten trials has already been referred to (Table 1). The curves plotted reveal the same characteristics (Fig. 3). The time curve begins much lower in the black-white maze but that is because of the fewer number of errors which decreases the total distance of the run. These curves do not show the actual number of trials nor the exact average time for any particular trial. The first two-thirds of the distance shows the learning process, the last third an automatism. The ordinary curve where one trial is the unit resembles this in main outlines but the automatic period is greatly extended. Curves plotted with one minute as the unit of time do not reveal really significant fluctuations since the maze can be run in ten seconds. An increase of ten or twenty seconds scarcely shows on the curve, although the rats are taking two or three times longer to run. The end of a graph, in which one trial is the unit, has been magnified and the last ten runs of the fifty are revealed in a more significant fashion (Figs. 4 and 5). The time curve for the normal maze is considerably lower than that for the black-white maze. The records show that the average speed of the last five trials of the rats in the normal maze is ten seconds better than the speed of the rats in the black-white maze for the corresponding trials.

Thus the evidence, from the behavior notes, from typical individuals, from the average speed in the first trials without error, from the actual speed of individual animals in the true path, from the numerical results and plotted curves, confirms the original assertion. Rats in the black-white maze, in experiments continued long past the learning period, maintain a slower speed than rats in the normal maze. The significance or cause of this will be discussed later. We will now consider the criterion of accuracy.

ACCURACY

One of the first things to attract the attention of the observer who was watching these experiments day by day was the very few errors which were made. No one who had had any experience with other mazes could fail to be impressed by it. A number of animals made their way around several times without error on the second trial. They went slowly, to be sure, but accurately. There were no signs of marked avoidance but neither was there any evidence of direct discrimination. Slow as these animals were they did not enter many *cul de sacs.* If a rat by chance entered one of these blind alleys it did not seem in any hurry to get out but neither did it linger in it. One rather expected that the rats would tend to hide or tarry in the side-paths, especially when the covered ways were the *cul de sacs.*

The accuracy was apparent from the very beginning and was not a virtue of slow growth. Whatever influence was at work it was there from the first. The rats on the normal maze made an average of 14.9 errors the first trial while those on the black-white maze made only half as many, 7.5.

Notice the difference in the initial height of the error curves of the two mazes (Figs. 2 and 3). The error curve of the black-white maze is not like any error curve made for a normal maze and it is a direct expression of the accuracy. The time curve which accompanies it, as has been said, is low because of the fewer errors and not because of more rapid speed.

There is a great decrease in total as well as in beginning errors from the mark set by the normal maze. The latter maze has to its credit an average of 66.6 errors per animal or 1.48 per trial for each rat while the black-white maze gives only 35.6 errors per animal or .8 per trial. The accuracy is nearly twice as great in the black-white maze.

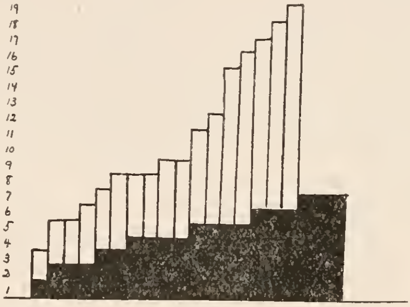

FIG. 6. Graph showing the point at which the animals made their first trial without error. Each vertical bar represents an animal. The black represents those of the black-white maze. This is superimposed upon the white, which represents the animals in the normal maze.

One would naturally expect then, what really is the case, that the error curve for the black-white maze (Fig. 3) would reach its lowest level much sooner than the error curve for the other maze—that the automatism would be more quickly established. The figures show that the rats on the black-white maze made their average first trip without error on the 4.2 trial but the average for this trip on the normal maze was 8.5. If we take the first ten trials and compare them we find that the rats in the black-white maze have an average of 4.2 perfect trips to their credit while the normal maze has only half that number, 2. The graph seen in Fig. 6 is made from the records of 20 animals in the black-white maze and of 17 in the normal maze. It shows the trial at which the zero point was reached and the number making it at each point. It is perhaps more striking than the ordinary error curve.

If the errors are so few in the beginning, if the co-ordination is acquired so quickly, why is it that there is not a greater difference in the total errors ? This is a question which requires an answer. The answer is that the final accuracy is less. See the curves, Figs. 2, 3 and 5. The normal error curve is regularly at the end of 50 trials below the black-white. What the causes are which produce this greater final variability is a question for our future inquiry. The three main points to be emphasized here are the few beginning errors, the rapid drop to the zero point and the persistence of errors to the very end.

SURPLUS TIME AND RATE OF ELIMINATION OF TIME AND ERRORS

Professor Carr argues at length for the value of the rate of elimination of surplus time and errors as one of the elemental components of the learning curve which varies independently from the other components.[15] The curves which show elimination in the experiments here reported are seen in Fig. 7. The rate of elimination is very similar in the two mazes. There are, however, some points of contrast.

The time curve for the normal groups exhibits a fairly steady rate of decline to the 24th trial. Here the final limit is reached, .4%. The time curve for the black-white group reaches its final limit of 1.8% on the 9th trial. It then follows the same course as the curve for the normal maze but with greater variability. The curve for the black-white maze reaches its level much sooner but cannot maintain it with the same constancy; neither can it reach, in the limits of the experiment, the same low level which the normal curve so easily reaches and maintains.

The error curve of the black-white maze indicates a much quicker rate of elimination than that of the normal maze. There is a steady drop till the 6th trial. From this point on there is only 6% of the errors left to eliminate. It will be remembered that the black-white maze had fewer errors to eliminate at the beginning. From the 6th trial on, however, the rate of elimination is below that of the normal maze. For the twenty succeeding trials the average is slightly above 6% and is only 1% less than this at the end.

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[15] Hicks, Vinnie, and Carr, H. A. Human Reactions in the Maze. *Jour. of Animal Behavior*, vol. 2, no. 2, p. 98. 1912.

The errors are eliminated more slowly in the normal maze. This curve does not reach the 6% level until the eleventh trial.

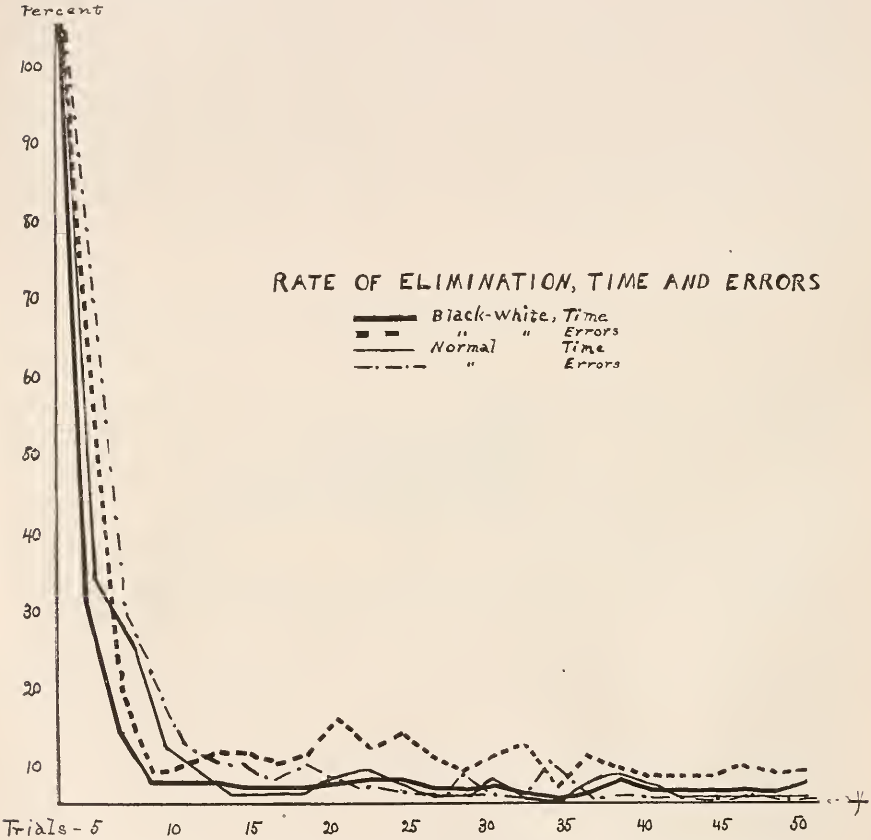

FIG. 7. Curves showing the rate of time and error elimination in the black-white and the normal mazes

From here on, however, there is a regular decline until, in the eighteenth trial, there is only 1% left to be eliminated. The curve wavers above and below 1% but this is practically the final level. The black-white maze has five times as much left to eliminate at the end as the normal maze.

BRIGHTNESS

But it may be argued that although the reactions of the rats in the black-white maze differ from those made in the normal maze in respect to speed and accuracy, the contributing cause may not have been the brightness of the runways. In reply it can only be said that no other difference can be seen in the experimental conditions. The maze was the same in every experiment and it stood in the same place. The animals were from the same breeding cages, were given the same care and were used by the same person throughout. Tests to prove that it was the brightness factor, however, were introduced.

The blind animals furnished the best means of control. These animals were of the same original stock as the others and during the entire period of experimentation were kept in the cage with the others. They ran the maze daily with the other rats. They were put through the box problem but could not learn this as the rats with vision did. When taken over to the maze they behaved just as the blind rats in the normal maze behave. The learning curve for these rats has been plotted and may be seen in Fig. 8. Compare this curve with that made for the rats in the normal maze, Fig. 2, and see how nearly identical they are. Neither resembles at all the curve for the black-white maze, Fig. 3. These blind rats were in good physical condition, active and strong, and the only observable difference from their companions was in their lack of vision. Is not the conclusion fair that the contrast in behavior and the different numerical results of the two groups of animals used in this part of the experiment were due to the visual situation in the maze ?

The second control was the use of the problem box. Rats taken from the box over to the normal maze showed no favorable effect of general training. The training if anything resulted unfavorably. The reasons have already been given. Rats trained on the box when taken to the black-white maze exhibited no unfavorable effects of training but gave some slight indications that the brightness experience had been an aid. The slightness of this effect was probably due to the differences in the essential nature of the two problems and the briefness of the time in which the animals were in the box.

From the black-white maze to the box, however, the effect was different. The experience certainly did carry over. The number of trials in learning was one-third less and the total number of errors was reduced in the same proportion. The brightness situation must have been responsible for these contrasting results. The lengthened period of training on the maze before attempting the problem box may account for its greater effectiveness in this case.

Although the conduct described above was influenced by brightness, may there not have been a preference, either instinctive or acquired, for black or for white ? It must be remembered that the animals in the experiments described may have been reacting to one color only regardless of any changes in the maze.

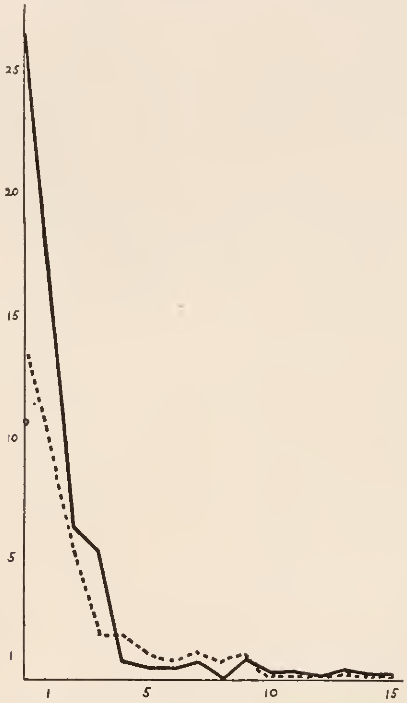

FIG. 8. Curves of the blind rats in the black-white maze
———— Time, - - - - - - - - Errors

We do not think the reactions were due to preference. If they had been, there would have been greater differences between the results of the experiments where the true path was white and those where it was black. The following figures, taken from many others, show the similarity of the conduct in the two cases.

	Black	White
Speed in first 5 trials	8.1 ± 4 min.	7.1 ± 3. min.
Final speed	.36 ± .1 min.	.44 ± .14 min.
Surplus time	46.4 ± 17.6 min.	47.6 ± .16 min.
Errors in first 5 trials	18.4 ± 4.4	16.5 ± 4.6
Total errors	30 ± 6.4	39.2 ± 10.5

There are variations, as will be seen in the above table, but the balance is now on one side, now on the other side of the scale.

If there were a preference for either black or white the observed behavior would have shown it. There would have been a lingering in one path rather than the other, a choosing or avoidance of the covered way, more errors in the one case than the other. There was no observable conduct which would indicate such a preference either for black or for white. There must be some other explanation than that of preference.

It is quite possible, even when it is granted that these results are not due to an instinctive preference, that there may have been a difference in the relative stimulative effectiveness of the black and the white paths. Since the effects of training were the same for both black and white the two groups may be compared directly irrespective of the minor grouping for training. The results are very similar. The differences are slight indeed in comparison with the larger differences which were seen between the black-white and the normal mazes and may be due to chance.

The white true path seems to give a better initial direction for the first trials. The animals constituting this group stayed in the true path more consistently than those of the group where the true path was black. But the black *cul de sacs* proved more attractive in the end, for while following the white path more total and more average errors were made than while following the black. A greater number of trials was also necessary in learning. The white path gave the lowest initial time and the the lowest final time although the total time of the two mazes was about equal. Thus while the white path probably gave a better beginning both in speed and accuracy and the speed as a whole was better, the record for total and final accuracy was less than that of the maze when the true path was black.

CONCLUSIONS

The conclusion seems justified that such a contrast in brightness between two roads is exceedingly effective with these animals as they learn the maze. It gives increased accuracy, as is shown both in initial and total decrease of errors, and in a decrease of total time. So far it seems advantageous, yet there was a final inaccuracy greater than normal and a lesser final speed.

There were no outward signs of discrimination such as marked avoidance, quick reaction, noticeable change of behavior with change of stimulus. There was no lingering in one path or another which might indicate an instinctive preference. There was only a slower, perhaps a more cautious, activity in which random movements were inhibited in both initial and early trials prior to any effects of learning.

If the difference is not due to discrimination, and we have no evidence from the behavior or the nature of the curve as ordinarily interpreted that it is, what has caused the different character of the learning ? All who have worked with animals know that a stimulus may be effective although not discriminated. In such a case as this, where two widely differing degrees of brightness were used, one may have been more directive, more potent, more attractive than the other. Professor Carr suggests the phrase " dominance of a stimulus." This would tend to attract the animal's attention either to the white or to the black pathway.

It was said above that the different behavior was seen " prior to any effect of learning." It must be remembered, however, that such a problem is solved not by one act but by a series of acts and hence there may be learning within the trial itself. If the second trial is influenced by the first, if learning has begun, where did it begin ? Learning the maze is not at all like a single act, jumping a certain distance, for example. In a problem of this kind, which takes so long a time, it clearly may begin in the first attempt. As the animals enter this maze they are in the true path be it black or white. The first error, to the right, is barred almost immediately, probably by kinaesthesis. It is seldom made again except in times of great confusion. The true path includes first, a run to the left half way across the maze, then a turn, and then a clear run across one side. In this side there is the possibility of an error, but not one into which the animal runs headlong. Hence from the very first there is a safe experience of a definite sensory sort which extends over a considerable period of time. Because it is the first experience, because it proves safe, because the animal, on the whole, is more in this path than the other, because his returns are made on this path, these may be some of the reasons why this path proved more potent, more dominant, more directive than the other and indicate one way in which learning may possibly begin.

The term learning needs more careful definition. Certainly there was little evidence of discriminative learning here. If there was any, it must have occurred chiefly within the first trial. This possibility may be referred to in a later paper. The curve showing the rate of elimination of time and errors furnishes a slight indication of learning to use vision in the interval between the fifth and the tenth trials, Fig. 7.

If the mere strength of the visual stimulus was the effective cause of the reinforcement or modification of the usual controls, then we shall have to conclude that the white path was, on the whole, the more dominant one. The entire matter then would hang upon the supposition that the brightness factor in a certain path had the power to hold or compel the attention of the animal.

The time taken per trial depends of course upon the speed and the accuracy. The more errors an animal makes, the more blind alleys he explores, the longer the time per trial. Thus the fewer errors would largely account for the lessened initial time. The contrast between black and white, as will be remembered, was as strong as could be produced. The final inaccuracies might have been due to this contrast effect which persisted to the end and attracted the attention of the rats when they were momentarily distracted and thus led them into errors. The stimulating power of this contrast was no doubt responsible for the late development of the kinaesthetic control. But the slow change, when it did come, from reliance upon vision to the automatism of kinaesthesis left vision free to be caught, to be attracted by these contrasts, and led the animals into errors.

The decrease of final speed might have been due to the greater number of errors, yet this is probably not sufficient to account for the result since the speed was less in cases where there were no errors. The slower speed was caused, perhaps, by a natural hesitation because of the attractiveness of the errors and this was made possible by the slighter kinaesthetic automatism.

Our final conclusion is then that if animals are given two contrasting paths side by side, differing in brightness, the one path may prove more dominant and favor accuracy and because of accuracy a shorter time in the early trials. After the problem is learned, in the slow turning over to kinaesthesis, when attention is freed, these sensory factors may still retain their potency in times of momentary distraction. The result is a less perfect automatism and a slower speed.

PRELIMINARIES TO A STUDY OF COLOR VISION IN THE RING-DOVE *TURTUR RISORIUS*[1]

ROBERT M. YERKES
Assisted by A. M. EISENBERG
From the Harvard Psychological Laboratory

At the present moment a thorough study of the visual reactions of a few types of birds and mammals is highly desirable. This paper presents an account of observations on the reactions of the ring-dove in the Watson-Yerkes color vision apparatus. The ring-dove was chosen as a subject because of its easy adaptation to laboratory conditions and its convenient size. It was hoped that it might prove an ideal bird for the intensive study of vision.

As a preparation for the study of color discrimination, the limits of the spectrum, and the stimulating values of various wave-lengths, observations were first made on the response of the bird to achromatic stimuli. The apparatus used throughout the preliminary work here reported was the Watson-Yerkes spectral color vision device, as described in volume one of the Behavior Monographs.[2] A Bausch and Lomb automatic arc lamp was used as a source of light, and a selenium cell, as described in the monograph (pp. 79-81) served as a means of measuring the energy of the stimuli employed. For the simple reaction-box shown as W in figure 7 of the monograph, the box represented in figure 1 of this paper was substituted, and instead of having two reflecting surfaces, M and L of figure 7 above referred to, fixed on the experiment-box and moving laterally with it, three reflecting surfaces were employed. These remained fixed while the experiment-box moved sufficiently to reverse the position of the two photic stimuli.

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[1] This work has been made possible by a grant from the Bache Fund of the National Academy of Arts and Sciences, which enabled the author to complete the construction of a spectral apparatus and install a selenium cell outfit to measure chromatic stimuli. Grateful acknowledgment is made to the trustees of the Fund and to the Committee in charge, for the facilitation of this research.

[2] Yerkes, Robert M., and Watson, John B. Methods of studying vision in animals. *Behavior Monographs*, 1911, vol. 1.

The general apparatus need not be redescribed in detail. The reader who is unfamiliar with it is referred to the above-mentioned monograph and to Watson's more recent book.[3] In brief, it consists of a source of light which, by means of a system of lenses, prisms, and slits, is made to supply chromatic stimuli in any desired quality or intensity. Two stimuli are presented to the subject simultaneously. The position of these stimuli may be reversed at the will of the experimenter. The subject is required to distinguish the stimuli and react differently to the two.

Assuming, now, that the reader has a general knowledge of the mechanism by which the chromatic stimuli are obtained, controlled, and measured, we may consider our method of procedure in its relations to the reaction-box of figure 1. This consists of an entrance chamber (A) in which, at the beginning of a series of observations, the subject is placed by the experimenter, and from which it passes, when the door (D) is raised, into compartment B, which may be designated the discrimination compartment. A sliding partition (M) enables the experimenter to avoid delay because of the unwillingness of the subject to enter B, for by raising the door (D) and drawing M slowly and steadily backward toward the rear of compartment A, the subject may, without disturbance, be compelled to enter the discrimination compartment. Once in B, the subject faces the two stimuli S, S. These are presented either with or without general overhead illumination, and they appear as illuminated surfaces, either chromatic or achromatic, 7 cm. long by 1.8 cm. wide. These two stimuli are separated by the partition P, of figure 1.

On the floor of each stimulus-box, E, are electrodes by means of which electric shocks may be given as punishment for failures to distinguish and properly to react to the two stimuli. The doors F, F, leading from the stimulus compartments into the alleys G, G, may be raised by the experimenter by means of the cords shown in the figure. When the subject enters the compartment which contains the stimulus selected by the experimenter as the positive stimulus, the appropriate door F is immediately raised, the slide-door, H, of the same side opened, and the subject thus permitted to pass by way of the alley G, back to the starting point at A, where is it allowed to feed for a definite interval. In case, however, the subject enters the other compartment, it is not allowed to pass into the alley, but instead, either with or without the use of the electric shock, according to the experimenter's previous decision, it is required to retrace its steps and again attempt to distinguish the stimulus which demands positive reaction from that which demands negative reaction.

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[3] Watson, J. B. Behavior. New York, 1914, p. 70.

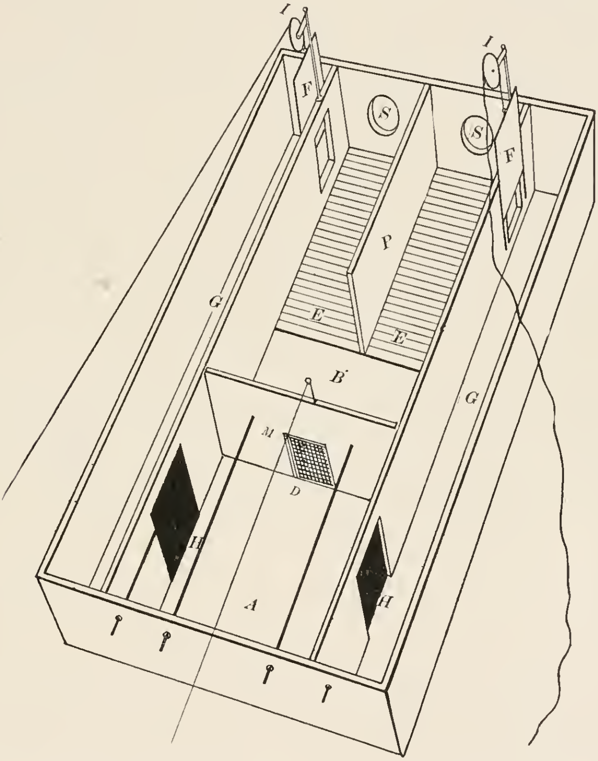

Fig. 1. Reaction-box for Ring-doves. A, entrance chamber; B, discrimination compartment; D, screen-door; M, sliding partition moving in A and B; E, E, stimulus compartments; P, partition between E and E; F, F, doors between stimulus compartments and alleys G, G,; H, H, slide-doors between G and A; I, I, pulleys for cords attached to F, F; S, S, stimulus apertures.

In the case of the observations about to be described, achromatic stimuli were obtained by placing a two candle power carbon incandescent lamp 86 cm. from the stimulus area. Thus, the one of the stimulus areas presented to the subject was always illuminated, whereas the other was entirely unilluminated, except as general illumination was employed in the experiments. Naturally, as the experiment-box was shifted from side to side, the more intense achromatic stimulus was presented now in the stimulus compartment on the right of the subject, now in the one on the left.

The writer is convinced that wherever possible the interference of the experimenter in the course of an animal's reaction should be obviated by the use of automatic or subject-actuated devices. It was not feasible, however, in the present investigation, to introduce such devices,—consequently the use of the slide-doors, as shown in figure 1. These, it should be stated, proved surprisingly satisfactory in the case of the ring-dove, which is easily startled and which would not react well in a subject-actuated apparatus unless everything could be made to work steadily, quietly, and fairly slowly. It is, however, beyond question that our efforts in studies of behavior should be to eliminate, as far as possible, the necessity, during the course of reaction, of movements by the experimenter which tend to modify the behavior of the subject. It has repeatedly appeared that even the experienced investigator is liable, unconsciously, to supply cues to his subject which facilitate proper reaction, or even serve as the sole basis for what appears to be discrimination.

The birds used for the present work were obtained from a Boston dealer. All that could be learned about them was that they were young We are therefore under the disadvantage of being unable to give a satisfactory description. It is obviously desirable in all such investigations that the origin and exact age, as well as the sex and history of each subject, should be known. But this is somewhat less essential, it must be admitted, in the case of preliminary observations than in that of continuation-work. Four birds were used. Of these, two, supplied as male and female by the dealer, in reality both males, appear as numbers 1 and 2 in this report. They were used over a period of several weeks by Mr. A. M. Eisenberg. The others appear as number 3, a female, and number 4, a male. During a period of five months these birds were used in the visual experiment by the writer. The results obtained with numbers 1 and 2 will be presented only in contrast with those of numbers 3 and 4, since the conditions of use varied somewhat, and the experiments conducted by Mr. Eisenberg were not carried so far as those of the writer. The descriptions of general behavior in this paper will be based almost wholly upon the observations made on doves 3 and 4.

At the outset, it was assumed that the ring-dove would react satisfactorily in the discrimination apparatus, that it would exhibit a fair degree of docility, breed rapidly in captivity, be easy to handle, and endure close confinement well. It must be admitted that these assumptions have not all been justified, for the birds did not quickly adapt themselves to the experimental situations, and in docility they rank low. Indeed, their slowness in acquiring the discrimination habit demanded in this work was a great surprise to the writer. He is now somewhat uncertain as to whether it is desirable to attempt an intensive study of visual response with a subject which demands such a large amount of training.

Work was initiated by feeding the birds in the entrance chamber of the experiment-box, with all of the doors of the box open so that the subject might wander about at will. This was continued for a week, with the occasional variation of opening and closing the doors as the bird passed from compartment to compartment, so that it might become accustomed to the operating of the simple mechanisms and learn the route from the entrance chamber, by way of the stimulus chamber, back to the starting point.

During the second week of the preliminary observations, the birds were sufficiently tame and accustomed to the apparatus to work fairly well. They were regularly each morning required to make the trip through the apparatus three or four times, and they were rewarded for so doing with food. It was discovered that they would not make the trip quickly unless they were very hungry, and even in that condition their attention to the situation was very variable, and they were so easily distracted by slight noises or jars that the whole process was a very tedious one. It thus became apparent that unless an additional motive for discrimination and progress through the experiment-box could be discovered, the work would be most tedious. Consequently, at the beginning of the third week, the electric shock was introduced as a means of compelling attention to the visual stimuli and of encouraging careful comparison and appropriate reaction. Even from the start, the electric stimulus served this purpose admirably. It at once rendered the birds more alert, careful, attentive, and active. The writer's notes record, " In two weeks the doves apparently have learned nothing, but to-day as the result of four trials with electrical stimulation, each seemed to attempt to discriminate between the light and the dark chambers.

It was decided, on the basis of the preliminary observations, that the doves should be required to choose the lighter rather than the darker of the two compartments.

Number 3, the female, was at the outset much less wild and more timid than number 4, the male. It was much easier for the experimenter to catch her in the living-cage than to catch him, but when in the experiment-box, she was very much more disturbed, excitable, and liable to discouragement than he. By contrast, then, the female may be described as tame and timid. the male as wild and bold. But it should be added that neither bird was sufficiently wild to be difficult to handle.

On February 28th, 1914, systematic, regular experiments were begun, with the use of both food and the electric shock. Both birds worked well in the six trials which were given. Only one bird was used at a time, and it was given its trials in succession, with from one-half to one minute interval for feeding between choices. In comment on this day's reactions, the writer's notes state that " The use of the electric shock discreetly and infrequently has transformed the birds from time-wasting and careless subjects to active, alert, constantly moving reactors. This modification of method evidently means a saving of an immense amount of time to the experimenter. It enables him to command the attention of his subject instead of having to beg for it by the offering of food. Food, however, is serving an excellent purpose in the work, for each bird comes to its task hungry and usually feeds between trials."

On March the 2nd, the number of trials for each bird was increased to ten, and it was subsequently found that as many as fifteen or even twenty trials could be given in succession without overfatiguing the subjects and with excellent results.

Table 1 presents two sample detailed records of the daily trials from the writer's note-book. The first portion of the table gives the results of an early series of ten choices, those of March 4th. The remainder of the table presents, by contrast, the results of a later series of fifteen trials in which the birds were practically perfect in their discrimination. This series was given on April 19th. The table indicates, in the first column, the position of the positive stimulus, that is the stimulus indicative of the chamber to be entered. In the second column, the letters R and W designate, respectively, correct and incorrect choices.

TABLE 1

EXAMPLES OF DETAILED DAILY RECORDS

March 4, 1914, 10:10 A. M. With general illumination. Stimulus-lamp 86 cm. from stimulus area. Coil at 1 cm. for female and 2 cm. for male.

		Female, No. 3		Male, No. 4	
Trial	Positive stimulus	Reaction	Remarks	Reaction	Remarks
1	Left	W	Shocked?	W	Shocked?
2	"	W	"	W	"
3	"	W	"	W	Shocked
4	Right	R	Direct	W	"
5	Left	W	Shocked?	R	Discrimination
6	Right	R		R	Anxious
7	"	W		R	"
8	"	W	Shocked	R	Eager
9	Left	R		W	No shock
10	Right	W		W	Shock
Summary:		4 R:6 R		4 R:6 W	

April 19, 1914, 9:50 A. M. With general illumination. Stimulus-lamp 126 cm. from stimulus area. Coil at 2 cm. for both.

		Female, No. 3		Male, No. 4	
1	Left	R	Near-mistake	R	Exc. disc.
2	Right	R	Direct	R	
3	"	R	"	R	
4	"	R	"	R	
5	"	R	"	R	
6	Left	R	"	R	
7	"	R	Good disc.	R	
8	"	R	"	R	
9	"	R	Near-mistake	R	Careful
10	Right	R	Eager	R	
11	Left	R		R	
12	Right	R	Direct	R	
13	"	R	"	R	
14	Left	R	Near-mistake	R	
15	"	W	Careless	R	
Summary:		14 R:1 W		15 R:0 W	

TABLE 2

SUMMARY OF RESULTS OF TRAINING IN LIGHT-DARK DISCRIMINATION.
ELECTRICALLY ILLUMINATED AREA VERSUS UNILLUMINATED AREA.
ELECTRIC SHOCK USED AS PUNISHMENT.

Dove Number 3, ♀				Dove Number 4, ♂			
Date	Conditions	Right	Wrong	Date	Conditions	Right	Wrong
Feb. 28	Gen. ill., elect. stim....	3	3	Feb. 28	Gen. ill., elect. stim....	4	2
Mar. 1	No gen. ill., elect. stim.	3	3	Mar. 1	No gen. ill., elect. stim.	4	2
“ 2	“ “ “ “ “	8	2	“ 2	“ “ “ “ “	5	5
“ 3	Mixed illum., “ “	9	1	“ 3	Mixed illum., “ “	5	5
“ 4	Gen. illum., “ “	4	6	“ 4	Gen. illum., “ “	4	6
“ 5	“ “ “ “	6	4	“ 5	“ “ “ “	6	4
“ 6	“ “ “ “	4	6	“ 6	“ “ “ “	7	3
“ 7	“ “ “ “	4	6	“ 7	“ “ “ “	6	4
“ 8	“ “ “ “	6	4	“ 8	“ “ “ “	4	6
“ 9	“ “ “ “	3	7	“ 9	“ “ “ “	5	5
“ 10	“ “ “ “	6	4	“ 10	“ “ “ “	7	3
“ 11	“ “ “ “	5	5	“ 11	“ “ “ “	4	6
“ 12	Mixed gen. ill. “ “	4	6	“ 12	Mixed gen. ill. “ “	9	1
“ 13	“ “ “ “	6	4	“ 13	“ “ “ “	8	2
“ 14	Gen. illum., “ “	5	5	“ 14	Gen. illum., “ “	8	2
“ 15	“ “ “ “	3	7	“ 15	“ “ “ “	6	4
“ 16	No. gen. ill., “ “	4	6	“ 16	No gen. ill., “ “	6	4
“ 17	Gen. illum., “ “	5	5	“ 17	Gen. illum., “ “	5	5
“ 26	“ “ “ “	3	7	“ 26	“ “ “ “	5	5
“ 27	“ “ “ “	9	1	“ 27	“ “ “ “	5	5
“ 28	“ “ “ “	4	6	“ 28	“ “ “ “	4	6
“ 29	“ “ “ “	6	4	“ 29	“ “ “ “	5	5
“ 30	“ “ “ “	4	6	“ 30	“ “ “ “	5	5
“ 31	“ “ “ “	4	6	“ 31	“ “ “ “	5	5
Apr. 1	“ “ “ “	11	4	Apr. 1	“ “ “ “	5	10
“ 2	“ “ “ “	9	6	“ 2	“ “ “ “	5	10
“ 3	“ “ “ “	8	7	“ 3	“ “ “ “	13	2
“ 4	“ “ “ “	9	6	“ 4	“ “ “ “	9	6
“ 5	“ “ “ “	8	7	“ 5	“ “ “ “	8	7
“ 6	“ “ “ “	12	3	“ 6	“ “ “ “	10	5
“ 7	“ “ “ “	9	6	“ 7	“ “ “ “	9	6
“ 8	“ “ “ “	11	4	“ 8	“ “ “ “	11	4
“ 12	“ “ “ “	11	4	“ 12	“ “ “ “	14	1
“ 13	“ “ “ “	13	2	“ 13	“ “ “ “	14	1
“ 14	“ “ “ “	14	1	“ 14	“ “ “ “	13	2
“ 15	“ “ “ “	10	5	“ 15	“ “ “ “	13	2
“ 16	“ “ “ “	12	3	“ 16	“ “ “ “	14	1
“ 17	Stim. less, “ “	11	4	“ 17	Stim. less, “ “	15	0
“ 18	“ “ “ “	13	2	“ 18	“ “ “ “	14	1
“ 19	“ “ “ “	14	1	“ 19	“ “ “ “	15	0
“ 20	“ “ “ “	12	3	“ 20	“ “ “ “	14	1

The general results of the several series of reactions required for doves number 3 and number 4 appear in table 2, under their appropriate dates. A brief statement is given in the second column of the table of the important conditions of reaction. It is stated, for example, whether general illumination was used

TABLE 3

Summary of Results of Training in Light-dark Discrimination. Electrically Illuminated Area Versus Unilluminated Area. Electric Shock Not Used

Dove Number 1, ♂				Dove Number 2, ♂			
Date	Conditions	Right	Wrong	Date	Conditions	Right	Wrong
Mar. 2	Gen. illum	3	7	Mar. 3	Gen. illum	2	3
“ 3	“ “	5	5	“ 4	“ “	2	3
“ 4	“ “	4	6	“ 7	“ “	2	3
“ 7	“ “	1	4	“ 9	“ “	2	3
“ 9	“ “	2	3	“ 10	“ “	2	3
“ 10	“ “	0	5	“ 11	“ “	2	3
“ 11	“ “	3	2	“ 12	“ “	2	3
“ 12	“ “	3	2	“ 13	“ “	0	5
“ 13	“ “	3	2	“ 14	“ “	2	3
“ 14	“ “	4	1	“ 16	“ “	2	3
“ 16	“ “	0	5	“ 17	No gen. illum	1	4
“ 17	No gen. illum	1	4	“ 18	“ “ “	2	3
“ 18	“ “ “	1	4	“ 19	“ “ “	2	3
“ 19	“ “ “	1	4	“ 20	“ “ “	1	4
“ 20	“ “ “	2	3	“ 21	“ “ “	2	3
“ 21	“ “ “	1	4	“ 23	No gen. ill., elect. stim.	1	4
“ 23	No gen. ill., elect. stim.	2	3	“ 24	“ “ “ “ “	2	3
“ 24	“ “ “ “ “	2	3	“ 25	“ “ “ “ “	1	4
“ 25	“ “ “ “ “	3	2	“ 26	“ “ “ “ “	5	5
“ 26	“ “ “ “ “	2	3	“ 27	“ “ “ “ “	4	6
“ 27	“ “ “ “ “	2	3	“ 28	“ “ “ “ “	4	6
“ 28	“ “ “ “ “	3	2	“ 30	“ “ “ “ “	4	6
“ 30	No gen. illum	4	6	“ 31	“ “ “ “ “	5	5
“ 31	“ “ “	5	5	Apr. 1	Gen. illum	5	5
Apr. 1	Gen. illum	2	8	“ 2	“ “	5	5
“ 2	“ “	6	4	“ 3	“ “	6	4
“ 3	“ “	1	9	“ 4	“ “	7	3
“ 4	“ “	4	6	“ 6	“ “	4	6
“ 6	“ “	8	2	“ 8	“ “	6	4
“ 8	“ “	5	5	“ 9	“ “	4	6
“ 9	“ “	5	5	“ 10	“ “	4	6
“ 10	“ “	2	8	“ 11	“ “	5	5
“ 11	“ “	5	5	“ 13	“ “	6	4
“ 13	“ “	5	5	“ 14	“ “	5	5
“ 14	“ “	4	6	“ 16	“ “	5	5
“ 17	“ “	7	3	“ 17	“ “	5	5
“ 18	“ “	5	5	“ 18	“ “	7	3
“ 20	“ “	2	8	“ 20	* “	4	6
“ 21	“ “	3	7	“ 21	“ “	7	3
“ 23	“ “	10	0	“ 23	“ “	3	7
“ 24	“ “	6	4	“ 24	“ “	10	0
“ 25	“ “	1	9	“ 25	“ “	5	5
“ 27	“ “	10	0	“ 27	“ “	9	1
“ 28	“ “	10	0	“ 28	“ “	6	4
“ 29	“ “	10	0	“ 29	“ “	9	1
“ 30	“ “	10	0	“ 30	“ “	5	5
May 9	“ “	6	4	May 1	“ “	6	4
“ 11	“ “	9	1	“ 2	“ “	10	0
“ 14	“ “	6	4	“ 2	“ “	10	0
				“ 4	“ “	10	0
				“ 9	“ “	9	1
				“ 11	“ “	8	2
				“ 14	“ “	5	5

or not, and it is indicated that in a few series of observations the conditions of illumination were mixed, that is, for some of the reactions general illumination was employed, whereas in others it was lacking. Throughout the regular experiments the electric stimulus was employed. On April 17th, as is indicated, the intensity of the visual stimulus was lessened, thus diminishing the difference in the stimuli to be distinguished.

Table 3 presents the comparable results for doves number 1 and number 2. The chief difference in the conditions for these results and those obtained with doves numbers 3 and 4 is the absence of the electric stimulus in the case of the former. With the exception of one week, March 23rd to March 28th, Mr. Eisenberg trained number 1 and number 2 to achromatic discrimination on the basis of food as a reward without the use of the electric shock as punishment for mistakes. His results, therefore, may be compared with those of the writer, with a view to discovering the value of punishment as contrasted with reward in this experiment with ring-doves.

Such comparison indicates, in the first place, that it is possible to make a larger number of observations per series with punishment than without it. Thus, the writer by the aid of the electric stimulus was able to make ten, fifteen or even twenty observations per series. Whereas, Mr. Eisenberg, without the electric stimulus, could not satisfactorily make more than ten observations, and during a considerable portion of the training he made only five. Second, the time required for the work varied much more widely when punishment was not used than when it was used. As appears from tables 2 and 3, all of the doves acquired the ability to discriminate with a reasonable degree of certainty, and to react appropriately. The course of habit formation in case of each of the four subjects is surprising. Instead of being steady, regular, and fairly rapid, as the writer had anticipated, it proved to be irregular and extremely slow. One day the experimenter would feel confident that his subjects were acquiring the habit, and the next day he would find them utterly unable to react properly.

In table 4 the choices are presented by groups of fifty, and the course of habit formation is indicated with the daily variations eliminated. This table shows that as the result of three hundred trials, no one of the four doves had acquired the ability

TABLE 4

REACTIONS IN LIGHT-DARK TRAINING GROUPED IN FIFTIES TO SHOW SLOWNESS OF IMPROVEMENT AND IRREGULARITIES

Trials	Dove 1, ♂		Dove 2, ♂		Dove 3, ♀		Dove 4, ♂	
	Right	Wrong	Right	Wrong	Right	Wrong	Right	Wrong
1 -50	18	32	18	32	32	18	27	23
51–100	18	32	20	30	23	27	29	21
101–150	22	28	21	29	23	27	35	15
151–200	23	27	28	22	25	25	25	25
201–250	20	30	25	25	27	23	22	28
251–300	25	25	26	24	30	20	29	21
301–350	37	13	36	14	34	16	34	16
351–400	36	9	41	9	41	9	46	4
401–450			27	8	38	12	47	3
451–500					30	5	33	2

to react properly. Between the three hundredth and the four hundredth trials, all of them, however, showed marked improvement. Were it not that two experimenters were involved and the conditions of observation thoroughly controlled, it might fairly be suspected that the doves finally discovered some other basis for reaction than the difference in the intensity of illumination. We are convinced, however, that this was not the case and that the results satisfactorily prove that the ring-dove is extremely slow, under the conditions described, in learning to react appropriately to achromatic stimuli, even though they differ very markedly. It must be admitted, however, that there are certain features in table 3 which are puzzling. Number 1 discriminated perfectly on April 23rd, and number 2 on April 24th, whereas on both the preceding and the following days they did poorly. This suggests to the writer that they had happened upon some means of choosing other than that intended by the experimenter.

From a careful comparison of the data of tables 2, 3, and 4, it is clear that by the use of the electric stimulus, it is possible to develop a visual discrimination habit in the dove much more quickly, and consequently with less labor, than by the employment of the food getting desire alone.

All of the foregoing observations are merely preparatory to the work with chromatic stimuli. It therefore seems unnecessary to burden the reader with further details of conditions or results, except possibly with respect to the general illumination and its relation to the reactions. In some of the series, general illumination was not employed, and it was naturally apparent that the doves could distinguish the stimuli much more easily than when the surroundings were illuminated. It was deemed desirable to use general illumination in order to guard against choice on the basis of the visibility of the sides and floor of the stimulus chambers, for naturally enough, this differed greatly in the light and the dark chambers in the absence of general illumination. On the whole, it seemed very much more satisfactory to conduct experiments in the general illumination produced by a two candle power frosted carbon incandescent lamp, at a distance of 110 cm. above the center of the partition beween the stimulus chambers.

As an aid to rapid reaction, the alleys of the experiment-box were kept dark except at the moment of entrance of the dove. In each alley was placed a low-power lamp which could be turned on the instant the door F was raised, and turned off the instant the door H was opened. This served to induce the dove to enter the alley-way and to hasten through it to the food-box. After a few daily series, the birds made the trip quickly and voluntarily, seldom loitering in the passageways and usually passing from entrance chamber to discrimination chamber rapidly.

The food placed in the entrance chamber as a motive for return to that portion of the experiment-box was milk-soaked bread, with a small quantity of cracked corn added. During a large portion of the series, the birds ate little, unless they were practically deprived of food while in the living-cage. It is thus fair to say that the process of habit formation in the case of doves 3 and 4 depended almost solely upon punishment, whereas the process in the case of birds 1 and 2 depended solely upon reward.

As in the writer's previous use of punishment, the induced current was used by means of a Porter inductorium with a number 6 Columbia dry cell as source of current. In the early experiments, no attempt was made to keep the feet of the birds moist, and as a consequence, the secondary coil had to be placed well over the primary. Its position was varied somewhat from day to day, but in general it was placed at 1 cm. for the female and at 3 cm. for the male. This, of course, means that the male responded to a very much weaker electric stimulus than did the female, but it is probable that this indicates not so much a difference in sensitiveness to the stimulus as the result of difference in weight, for the male bird was much heavier than the female. During March it was found difficult to get satisfactory responses, even when the maximum current was used, and the experimenter finally hit upon the device of placing a square piece of moist blotting paper before the food-box in the entrance chamber. This was found to yield very satisfactory results. The secondary now had to be set at 2 cm. for the female and 2½ for the male. The settings proved satisfactory throughout the remainder of the work, and whereas previously the responses to the electric stimulus had varied extremely, they subsequently were very constant.

RESULTS WITH CHROMATIC STIMULI

Doves 3 and 4, having been trained to practically perfect discrimination of a bright area from a dark area of the same size, were tested for preference of spectral red and green. The value of the red stimulus was 626 to 640$\mu\mu$, while that of the green was 498 to 510$\mu\mu$. In energy, as measured by the selenium cell, the red stood slightly above the green, but they were so nearly the same that it seemed needless to attempt to equate them more closely for these preliminary experiments.

Table 5 presents in summary the results of the chromatic reactions of doves 3 and 4. From this table it appears that on April 21st, when given an opportunity to choose either the red or the green chamber, without punishment, number 3 chose the one as often as the other, whereas number 4 chose the red eight times, the green twice. On April 22nd, in the absence of general illumination and with a period of two minutes for darkness adaptation before the series was commenced, the results were entirely different, for number 3 selected the green nine times out of ten, while number 4 chose it five times out of ten. On the following day, the original conditions of April 21st were reinstated and the responses were similar to those of that date. On April 28th, by the elimination of general illumination, darkness adaptation was effected, and the results again, as on April 22nd, favored the green.

TABLE 5

Results of Experiments with Chromatic Stimuli

Dove Number 3, ♀				Dove Number 4, ♂			
Date	Conditions	Red	Green	Date	Conditions	Red	Green
Apr. 21	Preference for red or green........	5	5	Apr. 21	Preference for red or green........	8	2
“ 22	“ “ “ .... (darkness adaptation)	1	9	“ 22	“ “ “ .... (darkness adaptation)	5	5
“ 23	“ “ “ .... (gen. illum.)	3	7	“ 23	“ “ “ .... (gen. illum.)	7	3
“ 28	“ “ “ .... (no gen. illum.)	4	6	“ 28	“ “ “ .... (no gen. illum.)	5	5
Apr. 29	Red-black training....	11	9	Apr. 29	Red-black training....	18	2
“ 30	“ “	16	4	“ 30	“ “	12	8
May 1	“ “	13	7	May 1	“ “	18	2
“ 2	“ “	13	7	“ 2	“ “	19	1
“ 3	“ “	10	10	“ 3	“ “	20	0
“ 4	“ “	17	3	“ 4	“ “	20	0
“ 5	“ “	18	2	“ 5	“ “	17	3
“ 6	“ “	17	3	“ 6	“ “	19	1
“ 7	“ “	19	1	“ 7	“ “	19	1
“ 8	“ “	18	2	“ 8	“ “	17	3
May 9	Red-green training....	14	6	May 9	Red-green training....	16	4
“ 10	“ “	10	10	“ 10	“ “	20	0
“ 11	“ “	10	10	“ 11	“ “	15	5
“ 12	“ “	13	7	“ 12	“ “	16	4
“ 13	“ “	15	5	“ 13	“ “	15	5
“ 14	“ “	13	7	“ 14	“ “	13	7
“ 15	“ “	14	6	“ 15	“ “	17	3
“ 16	“ “	14	6	“ 16	“ “	13	7
“ 17	“ “	14	6	“ 17	“ “	16	4
“ 18	“ “	13	7	“ 18	“ “	15	5
“ 19	“ “	16	4	“ 19	“ “	18	2
“ 20	“ “	16	4	“ 20	“ “	18	2
“ 21	“ “	15	5	“ 21	“ “	19	1
“ 22	“ “	17	3	“ 22	“ “	19	1
“ 23	“ “	14	6	“ 23	“ “	14	6
“ 24	“ “	14	6	“ 24	“ “	16	4
“ 25	“ “	18	2	“ 25	“ “	17	3
“ 26	“ “	18	2	“ 26	“ “	18	2
“ 27	“ “	20	0	“ 27	“ “	18	2
“ 28	“ “	20	0	“ 28	“ “	20	0

From these four series of ten reactions with doves numbers 3 and 4, it may be inferred that under the condition of general illumination in which these doves had been trained to distinguish the light stimulus patch from the dark and to react positively to the lighter of the two, the spectral red and green stimuli appeared of about the same intensity to the female dove, whereas to the male, the red appeared the more intense. One naturally infers that both birds, as a result of their previous training, would go to the stimulus patch which appeared the lighter of the two, supposing that an appreciable difference existed. The series of observations on April 22nd and 28th with darkness adaptation indicate that green appeared considerably lighter for both birds than without adaptation. Green was chosen more frequently by number 3 than by number 4, apparently because the two stimuli were of more nearly the same value in general illumination for this bird than for the male.

From these few observations, and naturally only a few observations could be made of preference, we may conclude that spectral red and green stimuli of approximately the same energy values did not appear markedly different to the female dove in general illumination, whereas without general illumination the green seemed the more intense. For the male, on the contrary, the red seemed somewhat more intense than the green, and darkness adaptation rendered the two of practically the same intensity.

Hess[4] has already demonstrated the Purkinje phenomenon in chickens and doves, by a method radically different from that of the writer, while Lashley[5] has more recently demonstrated it in the game bantam by the method of this investigation. There seems to be no reason for doubting that the observations described above also constitute a satisfactory demonstration of the modification of stimulating value by adaptation.

A series of observations was now instituted, beginning on April 29th, on the development of the ability to distinguish red from black and of the habit of reacting positively to red and negatively to black. Supposing that red appeared light and black dark, it would seem that both doves, merely as the result of their light-dark training with colorless stimuli, should select red uniformly and avoid the black. The results, however, as they appear in table 5, do not wholly justify this expectation.

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[4] Hess, C. Untersuchungen über das Sehen und über die Pupillenreaction von Tag- und Nachtvögeln. *Archiv. fur Augenheilkunde*, 1908, Bd. 59, S. 143.
——————. Vergleichende Physiologie des Gesichtssinnes. Jena, 1912, Bd. 4, S. 9.

[5] Watson, J. B. Behavior. New York, 1914, p. 350.

Instead, they seem to indicate that for the female dove, the red was so dark that it tended to be confused with the black, or at least was not accepted as the equivalent of the light area which the bird had previously learned to choose.

In this red-black training, it was possible to give each dove twenty trials in succession. As a result of one hundred and forty trials, number 3 was reacting properly ninety per cent of the time. Curiously enough, the male, number 4, chose the red eighteen times out of twenty in his first series, and showed throughout his reactions, in the red-black training, ability to respond to these two stimuli much as he had to the light and dark achromatic stimuli. This is, of course, wholly in agreement with the results of the preference tests, which clearly indicated that the red stimulus for some reason possessed a higher stimulating value for the male than for the female.

It is, of course, impossible to say, on the basis of the red-black results, that either bird responded to the chromatic difference instead of to the intensity difference of the stimuli. It is doubtless safer to assume that the latter alone was the basis of choice.

Beginning on May 9th both doves were presented with the red and green stimuli which on April 21st had been offered as a basis for preference reactions, with the difference that now they were required to choose the red and to avoid the green on penalty of electric stimulation. Again, each daily series consisted of twenty successive trials. The female exhibited, at first, slight ability to distinguish the two stimuli and to respond appropriately, but after three hundred and eighty trials, she was reacting perfectly. The male, on the contrary, reacted perfectly even from the first, his second series of twenty trials including no mistakes. It is thus fairly clear that he responded to the intensity difference of the two chromatic stimuli, and it seems wholly probable, in view of the gradual development of the habit, that she also acquired the ability to respond to the same difference.

From these preliminary observations, it seems safe to conclude that for the ring-dove a red and a green from the spectrum of the carbon arc, of the wave lengths designated above, and of approximately the same energy, as measured by the selenium cell, are sufficiently unlike in stimulating value to be readily distinguished by certain individuals and with difficulty by others. The particular results in hand suggest that the red has a higher stimulating value for the male than for the female.

The next step in the experiment would naturally enough have been observation of the responses of the subjects to varied energy values (intensities) of the two chromatic stimuli. Unfortunately, the investigation had to terminate at the end of May and the laborious preparation for these final observations was unavoidably wasted. The writer had fully expected and hoped, within the period of six months at his disposal when the investigation was undertaken, to ascertain whether the ring-dove can distinguish a red from a green stimulus throughout a wide range of energy or intensity values. This he did not succeed in doing, and consequently this report must be entitled "Preliminaries to a study of color vision in the ring-dove."

The principal conclusions which may safely be drawn from these observations have been suggested in the course of the presentation, but by way of summary and review, they may be enumerated here.

1. It is fairly obvious that the ring-dove is not sufficiently docile to be an ideal subject for the study of color vision by means of the method which Watson and I have developed.

2. It is indicated that the value of a certain red and a certain green may be very different for two ring-doves, and it is possible that this difference is correlated with sex, the red having a higher stimulating value for the male than for the female.

3. As has already been demonstrated by the writer in the case of a number of animals, the use of the electric stimulus as a means of compelling attention to an experimental situation and of promoting habit formation is desirable in work with the ring-dove.

4. Ring-doves differ markedly in temperament. The pair used by the writer throughout this work presented differences which must be considered if one is to understand the results. To begin with, the male was somewhat wild, but at the same time fairly bold, whereas the female was tamer but more timid. Because of this contrast in timidity, the male almost from the start proved the better subject. He was not so easily disturbed or distracted, reacted therefore more steadily, and chose more certainly. With constant handling he became quite as tame as

the female and lost almost entirely his timidity in the apparatus. She, however, continued to be rather timid throughout the several months of work, although she was perfectly tame. The differences in the nature of the reactions, as recorded in the experimenter's record-book, can be appreciated only in the light of these temperamental facts.

The sex contrasts indicated in the above paragraphs one dare not emphasize very strongly on the basis of observations on two individuals, but they at least suggest the desirability of further study of the sexes. It is the writer's opinion that they agree sufficiently closely with the results obtained in the case of other animals to justify their provisional acceptance.

As has been repeatedly noted with other animals, there are good and bad days in experimental work with ring-doves,—days which are good or bad, not, so far as one may tell, because of variation in the experimenter or his manipulation of the apparatus, but chiefly because of variations in the condition of the subjects. The experiments described in this paper were made at about the same hour each morning, and it was quite impossible for the experimenter to predict the outcome of a series in the light of previous series, for the attention of the doves to the situation seemed to vary independently of any conditions or group of conditions which the experimenter could take into account. There are animals which can be relied upon to work steadily and fairly predictably. The ring-dove is not one of them.

The writer has been led to reflect, because of the outcome of this series of observations, on the possible relation of the simplicity of the experimental situation to the results. He was compelled to devote several weeks to the establishment of a simple habit in two ring-doves, a habit which was next to valueless except as a preparation for further observations. It is natural that during this long period of preparation he should frequently wonder whether the desired end might not be gained more quickly by a different method. It seems probable that a complex situation would have proved more favorable, and that had the two stimuli varied in other respects than in intensity, the animal's attention would more readily have been directed to them and more steadily held upon them. The matter is mentioned here because it is obviously of extreme importance to students of behavior to discover the most efficient means of developing preparatory habits in animals.

In concluding this paper, the writer can not refrain from calling attention to the waste of time which results from the sacrificing of trained animals at the end of an investigation. It should be possible, through exchange, to make the same subject serve in various experiments. And different experimenters, supposing our methods to be reasonably standardized, might study quite different problems on the basis of similar preparatory habits. Thus, for example, the doves which in this investigation have been trained to certain visual reactions, might perfectly well be employed for other forms of visual response, or even to greater advantage for studies of the relation of the central nervous system to the acquired responses. It is suggested, therefore, that American investigators who are actively engaged in studies in animal behavior keep in close touch and develop a system of reporting their experiments while in progress, which may serve as a basis for the serviceable exchange of trained subjects. The writer happens to have on hand at the moment of writing three tame crows which are highly trained in certain modes of response. The labor of taming and training them would have to be valued at several hundred dollars. It is impossible, under present conditions, to make use of these birds, and unless some other investigator can be found who can take advantage of this preparation, they will have to be either set at liberty or otherwise sacrificed.

THE BEHAVIOR OF BROOK TROUT EMBRYOS FROM THE TIME OF HATCHING TO THE ABSORPTION OF THE YOLK SAC

GERTRUDE M. WHITE
Zoölogical Laboratories, University of Wisconsin

WITH FOUR FIGURES

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A. INTRODUCTION

Although certain senses of mature fishes have been carefully studied, little work has been done upon the reactions of embryos. The investigations of the adult fish have been made chiefly with reference to the senses of smell, taste, sight, and hearing. Herrick ('03) found that some fishes possess taste buds located in the skin, by which they habitually discover their food, while other fishes have the sense of taste confined to the mouth. That the catfish has a true olfactory sense, which is distinct from gustatory, was shown by Parker ('10). The sense of hearing has also been studied by Parker ('05, '08, '11), who believes that some fishes are stimulated by sounds of slow vibration. Bernoulli ('10), on the other hand, maintains that the fishes with which he worked do not hear, but respond through tactual and visual stimulation, when at all, to the mechanical motion of the water.

The work of Paton ('07), who describes some of the reactions of fish embryos, is of particular interest. He believes that the early attempts of fish embryos to lie upon the ventral side are not due to the influence of the nervous system, but rather to the position in which they lie in the egg, and the shape of the body, combined with the propulsion of the water. Even embryos of thirteen and fourteen millimeters show a tendency to right themselves when they swim.

This author found that trout embryos of fifteen millimeters often swam once or twice around a dish five centimeters in diameter, but in all fishes progression was possible long before this, even at nine or ten millimeters. Squeezing or pricking the yolk sac of the trout caused exaggerated movements in the young fish. The head was found to be less sensitive to touch than the body; the eye in all stages examined was rather insensitive to touch. Prompt and unmistakable responses to thermal stimuli appeared at an early age. The rate of the heart beat at ten millimeters was twenty-five or twenty-eight, and at fifteen millimeters was seventy-five or eighty.

Since the information concerning the reactions of fish embryos is so meagre, it seems desirable that the kinds of stimulation to which various types of fish react, and the age at which such reactions begin, should be ascertained and possible applications to economic problems considered. The present paper is an attempt to give a connected account of the life activities of the Brook Trout, *Salvelinus fontinalis*, from the time of hatching to the absorption of the yolk sac.

The Brook Trout is usually found in clear, cold spring water, and prefers brooks or streams flowing over gravelly bottoms. It pushes from the rivers into the smaller streams, seeking the head-waters, where it rests in the deep pools and eddies. Under natural conditions it is seldom found in water over 60° F. to 65° F. The Brook Trout spawns in autumn as the temperature of the water falls. The season, which usually lasts about two months, begins earlier in the northern latitudes, in the Lake Superior region in September, or even in August, while in New York, New England, and Lower Michigan, it commences about the middle of October. The time necessary for developing the eggs is dependent upon the temperature of the water, varying from about 125 days at 37° F. to about 50 days at 50° F.

The experiments here described were performed in the Zoological Laboratories of the University of Wisconsin. About five hundred embryos were used, eggs being obtained at the Madison Fish Hatchery, and brought to the laboratory in four different batches, so that embryos of various stages could be observed at the same time. The youngest stages were kept in a wire tray in a trough of running water (from Lake Mendota) while the older individuals were placed in glass dishes and set in running water to keep them cool. About one hundred Rainbow Trout, *Salmo irideus*, of two different stages and a number of young German Brown Trout, *Salmo fario*, which swam freely in the trough, were kept under observation.

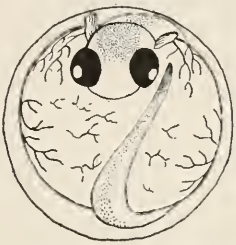
During the experiments the temperature of the water ranged from 5° C. to 10° C. Most of the experiments were performed in a dark room, the temperature of which was usually about 19° to 20° C. The fish were handled with a feather or with a spoon made of bent wire with netting stretched across it. Although there was a high rate of mortality (due to gas-bubble disease, fungi, algae which crept into the gills, and persistent handling), several of the Brook Trout survived during the whole period. All experimental data refer to the Brook Trout unless otherwise stated.

This work was accomplished under the direction of Professor A. S. Pearse, for whose helpful suggestions and encouragement it gives me great pleasure to express my appreciation.

B. EXPERIMENTS AND OBSERVATIONS

1. *Hatching*

The egg of the Brook Trout is small and nearly colorless, measuring about four millimeters in diameter. The embryo, which is about three times as long as the diameter of the egg, lies curled around its yolk sac with the tip of the tail beside the head. The eyes and head are visible through the thin shell. The hatching is initiated by movements starting at the head and later extending through the whole length of the body, so that the position of the embryo in the egg is somewhat changed. Such movements continue at intervals, varying from a quarter of a minute to an hour or more, until the shell is so strained that a slit appears. There does not seem to be any distinction as to which part of the embryo comes out first, for in the twenty-three cases observed, eight embryos appeared head first, one tail first, and in fourteen cases the yolk sac broke through before the body. The final shedding of the egg case is sometimes brought about by a violent movement of the body, but in nearly all the instances observed it was a gradual process lasting from three-quarters of an hour to five or six hours, during which the initial slit was slowly enlarged by the rhythmical motions of the body and the respiratory movements. If the anterior end is to be the first to appear, the violent contraction of the embryo raises the head until the strain splits the shell far enough to free the region bearing the pectoral fins, which immediately begin to move. The front of the head soon follows. Whether the tail or the yolk sac comes out first, the length of time required is



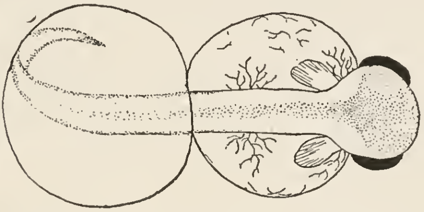


Fig. 1. Egg of a Brook Trout shortly before hatching. Magnified six and two-third times. (Drawn by Miss Wakeman)

Fig 2. Embryo hatching head first. Magnified six and two-third times

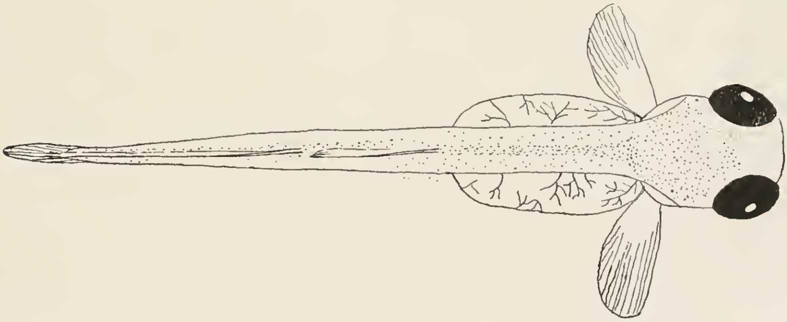


Fig. 3. Dorsal view of an embryo three weeks old (15 mm.). Magnified six and two-thirds times

nearly the same, and the details of the process differ very little. Unless the animal is disturbed, it may lie quiet for hours while the egg case slips off, but if the fish is jarred, a violent contraction almost invariably results, thus hastening the loss of the egg case. In nature, where there are great numbers of fish hatching simultaneously, they undoubtedly touch one another continually, shortening the time required for hatching.

The Brook Trout makes its appearance as a pretty, delicate, translucent creature about twelve centimeters long. Its most conspicuous features are its enormous eyes, which occupy almost the whole head, its huge yolk sac covered with a fine network of bloodvessels leading to the heart, the beating of which can plainly be seen. Along the back is a strip of pigment extending from the head to the tip of the slender tail.

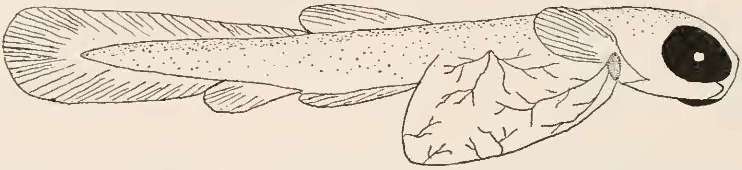


FIG. 4. Lateral view of an embryo three weeks old. Magnified six and two-thirds times

2. *Swimming Movements*

Although the Brook Trout spends most of the first six weeks of its larval life lying quietly on its side, it is perfectly capable of swimming as soon as the egg case is lost. In fact, Paton ('07), who worked on trout among other varieties of fish, states that definite progression is possible when the embryo is nine or ten millimeters long. In the present experiments, if a fish which had just hatched was suddenly touched, it would whirl round and round as if its cumbersome yolk sac formed a movable pivot. By the fourth day the movements are still rotatory, but the fish swim in larger circles and can go straight ahead for a greater distance. There is also more darting about, the tail being always the most active part of the body. The trout which is one week old (14-15 millimeters) swims in a spiral course. As the yolk sac diminishes in size, the fish is better able to control its movements. It lies upon its ventral side like an adult at the age of six weeks in most cases, and in swimming, continually darts about, turning first one way, then the other.

3. *Reaction to Mechanical Jars*

Possibly the first stimulus to which a developing trout reacts is that of mechanical jars. Often before the embryo has completely left the shell, the shaking of the dish or currents in the water cause it to contract, and if the tail is free, to swim about with the head still encased. This sensitiveness to mechanical vibrations continues throughout the larval life. Even so slight a vibration as that caused by blowing on the water makes the fish dart about.

4. *Reactions to Touch*

The sense of touch is well developed when the trout hatches, but it is impossible to predict what response will be given to a stimulus in any particular part of the body. Embryos in nearly all stages of development from the time of hatching until the yolk sac was lost, were touched systematically with a slender, pointed stick, and with bristles to determine whether one part of the body was more sensitive than another. I found, as Paton ('07) did, that although it is very difficult to localize the tactile areas, the head is much less sensitive than the body, and the eye is quite insensitive. Judged by such responses, the tail seems to be the most sensitive part of the body. It was found that the trout avoided a brush much more vigorously than they did a pointed stick or a single bristle. This is probably because the brush stimulates them at more points.

If touched persistently the embryos swim about, turning rapidly in all directions. When they are eventually fatigued, the responses become less marked, the trout may then merely move its tail or increase the rate of movement of its fins. It may even become absolutely quiet for a time, after which the reactions take place as before. The young trout show some measure of adaptability, for they grow somewhat accustomed to being handled, and particularly in the case of the oldest fish (three months old), those which had been repeatedly picked up were slightly easier to catch than those which had never been touched.

5. *Reactions to Current*

Since Brook Trout live in swift flowing streams, one would expect them to react to currents. In order to test this matter, a trough was constructed in which they could be tested. The apparatus was fifty inches long by two and one-fourth inches wide and three inches deep with straight sides. A piece of rubber tubing connecting with a faucet was attached to the center of one end, in such a way that a current of uniform intensity flowed through the whole length of the trough; at the other end, was an outlet covered with netting to prevent the loss of the fish.

After testing various strengths of current, the one which brought about the greatest number of reactions was found to be that which carried carmine solution the length of the trough in half a minute. Therefore, this strength of current was used in most of the experiments. Since all tests were made in a dark room, it was a simple matter to turn on the faucet a certain distance with the room in total darkness, in that way eliminating all reactions to light. Nevertheless, since it was found that the daylight had little effect on the results, the room was not darkened for all the experiments.

Nearly all stages of young Brook Trout from the time of hatching to the absorption of the yolk sac were tested. Table 1 shows some of the results.

TABLE 1

SHOWING THE REACTIONS OF BROOK TROUT TO CURRENT

Number of experiment	Number of fish used	Age of fish	Condition of light	Number of fish positive	Number of fish negative	Number of fish indifferent	Per cent of fish positive
1	25	4 days	day	22	0	3	88%
2	25	4 days	dark	21	0	4	84%
3	20	34 days	day	17	0	3	85%
4	20	42 days	day	16	0	4	80%
Total percentage of Brook Trout reacting positively................							84.25%

The fish were placed in the trough one at a time in most cases, with right and left sides alternately toward the current before the water was turned on. The fact that the older trout showed a slightly smaller percentage of positive reactions is probably not significant, as there appears to be no difference in their reactions, except such as would be caused by their greater activity and strength. Although it is not recorded in table 1, embryos which had just been hatched were found to react positively to current. But they were usually unable to swim more than a few centimeters, or able merely to orient themselves, owing to the size of the yolk sac.

While none of the Brook Trout were persistently negative in their reactions to current, there were always a few which did not give a definite positive response. Since it was noted that a fish was sometimes carried backward farther than it was able to advance, the four embryos in experiment number two which did not react definitely were tested later in the light, where they could be watched. One fish did not swim at all. Three fish were carried backward, but oriented toward the source of the current. They struggled to advance, but were unable to do so. Therefore, these were actually positive.

Of the twenty-one which are marked positive, five were evidently weak; their condition somewhat resembling the three just mentioned. Since these were found on being tested in the light to be clearly positive, and on the third trial in the dark progressed decidedly toward the current, they were included with the trout which had just been observed to move definitely against the current.

In order to discover whether surrounding objects affect the reaction to current, a striped paper was passed back and forth, beneath and at the sides of the glass dish containing Brook Trout about three weeks old. No reaction resulted. Brook Trout were also placed in a round glass dish set within another dish and a current of water made to run between the dishes. To this also the fish failed to react. Hence during the first few months, the sense of sight appears to have little or no relation to the reaction of Brook Trout to current. This does not agree with what Lyon ('04) believed to be true regarding the fishes with which he worked.

Positive rheotropism was also exhibited by a school of German Brown Trout two or three months old, that were almost invariably found resting or swimming about near the source of the current in the trough where they were kept. On testing Rainbow Trout three days old, when the yolk sac is enormously large, forty or fifty per cent were observed to be positive to current, while sixty per cent were indifferent, the latter either lying quiet or whirling about without orienting themselves when stimulated.

6. *Reactions to Light*

That young Brook Trout are negatively phototropic has been recognized by the fish-hatchers who, finding that the trout seek the dark corners, keep the troughs covered. Their phototropism, however, was tested more accurately in the following manner: A Nernst lamp was placed within a large box (75 cm. wide x 120 cm. long x 120 cm. high) blackened inside and out, and having an opening (6 cm. x 9 cm.) at one end. A narrow glass dish, 4 cm. through, containing water was placed before the hole to absorb the heat rays; at right angles to this was an oblong glass dish with rectangular sides (38 cm. long x 10 cm. wide x 8 cm. high) in which the fish were placed.

Brook Trout of nearly every age, from those which had just hatched to those two months old, were placed in the dish singly or in groups and left for varying lengths of time. Table 2 shows the results.

TABLE 2

Showing the Light Reactions of Brook Trout

Age of trout	Number of trout	Strength of light	Number of trout negative	Number of trout positive	Number of trout indifferent	Per cent of trout negative	Per cent of trout positive	Per cent of trout indifferent
2 days	25	1.5 candle meters	14	0	11	56%	0	44%
2 days	25	2.3 candle meters	21	2	2	84%	8%	8%
2 days	5	7.7 candle meters	4	0	1	80%	0	20%
21 days	10	2.3 candle meters	7	2	1	70%	20%	10%
21 days	10	7.7 candle meters	8	0	2	80%	0	20%
21 days	10	16 candle meters	8	2	0	80%	20%	0
Totals.....	85		62	6	17	75%	8%	17%

In the experiments tabulated the trout were placed in the center of the dish with right and left sides alternately toward the light, so as to eliminate complication from a propensity to turn toward a particular side of the body. Each fish was observed for five minutes. The strengths of the lights given are approximately what the fish actually encountered in the center of the dish. It may be noted that a somewhat greater percentage reacted negatively to a light 2.3 candlemeters than to a light 1.5 candlemeters. Above 2.3 candlemeters, however, the increase in the strength of light seems to make little difference. Brook Trout less than a week old in general react more strongly to a weak light (2.3 candlemeters) than to a strong one (16 candlemeters). With the older fish this does not seem to be true.

The conclusion that young Brook Trout larvae are negatively phototropic was corroborated by other incidental observations. When a dish containing fish was placed before a window, they almost invariably sought the side of the dish away from the light. The same was true of the Rainbow Trout.

In order to discover whether a Brook Trout is photokinetic, a Nernst lamp was suspended about eighteen inches above a dish containing them. When the light was first turned on, the trout darted about vigorously, many seeking the corners of the dish. After a few minutes' exposure, however, they came to rest quietly as before. The same experiment was tried with Rainbow Trout with like results. It was observed that the raising of a window curtain suddenly allowing the sunlight to fall on a dish of Rainbow Trout, stimulated them to unusual activity for several minutes.

From these experiments it is evident that Brook Trout are photokinetic and negatively phototropic.

7. *Light and Current*

It has been shown that Brook Trout are negative to light and positive to current. It is desirable to know how they react when the two stimuli conflict. Fish-hatchers claim that when a light is placed at the head of the current, the trout go away from the light, thus reversing the usual reaction to current. In order to study this matter, the trough used for the current experiments was entirely covered except for an opening 3 cm.

x 4 cm. at the end where the water entered. Opposite this opening was placed a Nernst lamp having an intensity of about ten candlemeters in the center of the trough.

Trout about one week old were placed in the center of the trough, four at a time in most instances, since the trough was found to be large enough to hold that many without interference. Their positions were noted just before the water was turned on and again five minutes later. Twenty-five Brook Trout were used. Of these twenty-three moved away from the light and two fish did not change their positions.

This seems to show that Brook Trout become negative to current when a light is placed at the head of the stream. This probably means that in natural conditions, when they have to choose between shelter and cool water, they seek shelter.

8. *Carbon Dioxide and Light*

Since an excess of Carbon Dioxide is a condition which a growing fish is likely to meet, the way in which a Brook Trout responds to it and the effect it has upon its light reactions is very important in determining whether or not the trout is to escape from the unfavorable conditions, and swim into purer water, where there is more oxygen.

For the purpose of discovering the effect of an excess of carbon dioxide upon the response to light, fifty individuals were tested separately in the apparatus used for the light experiments in a five per cent solution of carbonated water.[1] The strength of the light for one-half the fish was two candlemeters, for the other half about eight candlemeters. The results of the two were similar.

Almost immediately after a trout was placed in the carbonated water, the fins began to move more rapidly and the mouth to open and close with a gulping motion. Shelford ('14) describes this same condition in the fish that he tested in an excess of carbon dioxide. In the present experiments, the trout were stimulated to great activity, swimming continually from one end of the dish to the other with no apparent reference to the light. They were as likely to stop at the end of the dish toward the light as at that away from it. Excess of carbon dioxide apparently causes the Brook Trout to be indifferent to light.

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[1] The solution of carbonated water was made by adding carbonated water from a siphon to the ordinary city water, which is taken from Lake Mendota.

A ten per cent solution of carbonated water was also tried, but this was found to partially anesthetize the Brook Trout in from three to five minutes, and therefore no reactions resulted. The young trout were able to endure a five per cent solution for two or three hours without apparent injury; at the end of such a period of time they seemed somewhat sluggish. A two and a half per cent solution also had a stimulating effect. A twenty or twenty-five per cent solution caused all swimming movements to stop almost immediately, and made the heart beat of a trout three weeks old fall within about eight minutes from an average of eighty beats a minute to thirty-seven. Death ensued very soon after that. This was tried in several other cases with similar results.

9. *Reaction to Shadows*

As was stated in the discussion of rheotropism, the Brook Trout three weeks old does not respond to moving objects outside the water. This seems to be true of objects in the water also, provided they do not cause mechanical jars. This absence of reaction to shadows continues until the embryo is about six weeks old, when the greater part of the yolk sac is absorbed. At this time the trout suddenly begin to respond; the waving of a hand above the dish causes them to dart about in all directions. If such a movement is made repeatedly, however, they soon become accustomed to it and cease to react for a time.

The reaction to shadows is, therefore, not present at hatching, but becomes apparent at the time when the yolk sac is greatly reduced in size and shortly before the feeding reactions begin. It would be interesting to know whether there is a change in the eye or the nerve connections at this period, which brings about this new response, or whether it is merely due to increased swimming power.

10. *Feeding Reactions*

The feeding reactions begin when the Brook Trout are about two months old. At this time the larvae appear to develop a sudden curiosity concerning everything about them. They swim to the top more frequently and often explore the bottom. The fish studied were fed liver chopped very fine and put into the water with a dropper. For several days the trout did not appear to notice it, at least they were not observed to eat. A stream of meat juice directed against the body was avoided in the same manner as a jet of clean water. After a week or less, however, the trout began to take bits of food into their mouths as they chanced upon them and often to swallow them. From this time on they were observed to dart after pieces of meat floating about in the water, although they often rested directly upon meat lying on the bottom without appearing to pay any attention to it. They were also seen to chase bubbles and bits of filter paper, and to take them into their mouths, but they never swallowed them. The fish were fed in dishes with black or white bottoms. The trout were found to take food more eagerly from the dishes with black bottoms where the food was more plainly visible, although they would also eat pieces of meat over the white backgrounds. This fact is made use of by the fish-hatchers who feed the larvae in blackened troughs.

In order to discover what part is played by the chemical sense in helping the Brook Trout to find its food, a bag containing meat was placed in the water; this was nosed by the trout and one fish bit at it. At other times two bags, one with food and one without it, were set in the dish. The trout investigated both bags, but they bit at neither. They were apparently unable to discover meat hidden under a paper in the bottom of the dish. Although they wandered over it as they swam about, it was not noted that its presence had any effect upon the fish.

The Brook Trout apparently first react through sight to the presence of food, since they were often observed to leave pieces of meat near them to dart after bits farther away, which would not be the case, were it the chemical sense which was most strongly stimulated. The gustatory sense appears to determine whether or not the food is swallowed.

C. GENERAL DESCRIPTION OF THE EARLY LIFE OF THE BROOK TROUT

Let us follow a developing Brook Trout on the pebbly bottom of a swift flowing stream. During the first six weeks of its existence it does not move far from the spot where it was hatched, but lies quietly in the shadows among the stones, out of sight of its enemies. It is not affected by objects passing over head.

If someone throws a stone into the water the jar startles the little fish into swimming about rapidly for a few seconds, after which it sinks again into some shady nook. Here it rests, until a sudden eddy caused by an animal swimming through the water, makes it dart a few inches into the current. Other tiny fish touch and jostle it continually. If the water becomes filled with carbon dioxide, it becomes more active, and overcoming its impulse to avoid the light, swims about restlessly from place to place until it comes into purer water, where it again sinks down beneath the stones. Thus far its responses have been largely avoiding reactions, serving to keep it from unfavorable conditions.

When the trout is about six weeks old, it becomes more sensitive to objects outside itself. The sight of other animals passing by sends it scurrying under the cover of moss and stones. Shortly after this it begins to be curious, nosing nearly every object which it sees. It swims to the top of the water in pursuit of a bubble. It explores the bottom of the stream, often swimming head downward, passing in and out among the rocks, stones, and algae. Many particles on the bottom or floating above are taken into the mouth. If found to be good to eat, they are swallowed; if not, they are expelled. As the fish eats, it takes food more and more eagerly until it is satisfied, when it ceases to react, and hides in the algae.

Throughout its larval life the Brook Trout is reacting to external and internal stimuli, responding to nearly every current, object, or ray of light that strikes it.

In general, the behavior suits the needs of the fish. As long as the trout are very young, and are encumbered with the large yolk sac, which renders them unable to swim any distance, their reactions are such as would naturally tend to keep them lying quietly out of the sight of their enemies. They exhibit no curiosity, but avoid the light, hiding beneath the rocks and stones, reacting to current just enough to keep them from being carried down stream. The trout appear not to notice external objects, except as their approach jars the fish, making them struggle to regain their equilibrium.

As was previously stated, an excess of carbon dioxide renders them temporarily indifferent to light, so that they swim about restlessly until they reach a place where the water is purer.

When the Brook Trout grow larger, lose the yolk sac, and become strong enough to escape their enemies by swimming away, they begin to notice moving objects inside and outside of the water. The approach of any object sends them darting about in all directions in search of a hiding place. Just before the trout are old enough to commence eating, they show great interest in every object in the water, and begin to try taking any small object from a bubble or a bit of alga to a piece of meat into their mouths, though they appear to swallow only such as are edible.

From the consideration of the facts of behavior one is naturally led to ask what are the artificial conditions which best suit the needs and instincts of the young trout. In other words, what is the economic importance of the experiments discussed in this paper. One easily concludes from the observation of the natural conditions of Brook Trout and their reactions to current and carbon dioxide, that the first essential is cool running water with plenty of oxygen. The water should be free from algae of a sort which is apt to get into the gills. If a fungus attacks the young trout, the disease spreads rapidly, unless the infected and dead fish are removed, since the fish knock against each other as they swim about.

The fact that Brook Trout are so strongly negative to light seems to indicate that hatching troughs should be covered, or if the fish are in ponds or streams, that the trout should have natural covers, such as rocks, stones, or water plants, under which to hide. By living beneath these they may often escape predaceous animals which prey upon them.

Since it requires nearly a week for Brook Trout to learn to eat, they should be carefully watched when they are near the feeding stage, for if they do not learn to take food before the yolk sac is entirely absorbed, they will die of starvation. Shortly before the trout are two months old, they commence to swim to the top frequently and to exhibit curiosity, which indicates that they will soon begin to eat. Meat ground or chopped very fine should then be introduced into the water, so that the fish may take particles of it into their mouths by chance, as they wander about, and thus become accustomed to it before it is necessary for them to eat.

D. SUMMARY

1. The Brook Trout which has just hatched swims with a whirling movement. About the fourth day after hatching, the trout commences to swim in a spiral course, and from then on, the movements become gradually better co-ordinated, the trout swimming in larger circles and going straight ahead for greater distances.

2. The Brook Trout reacts to touch and mechanical jars immediately after hatching. The head is the least sensitive to touch of any part of the body, the eye being insensitive. The reaction is more marked when the trout is stimulated at a number of points, than when it is touched with a single bristle.

3. Positive rheotropism becomes apparent as soon as the trout has hatched.

4. The Brook Trout is photokinetic and negatively phototactic.

5. Directive light from a lamp at the source of the current reverses the usual rheotropic reaction, showing that Brook Trout are more strongly negative to light than they are positive to current.

6. An excess of carbon dioxide up to a certain point stimulates Brook Trout; a very strong solution depresses them. A five per cent solution stimulates trout to move about continually and makes them indifferent to light. Stimulation is also brought about by a two and a half per cent solution. A twenty or twenty-five per cent solution causes a rapid fall in the rate of the heart beat, then death.

7. Brook Trout begin to respond to shadows about the fifth week after hatching, when the yolk sac is greatly diminished in size.

8. Feeding reactions commence when Brook Trout are about two months old. The sense of sight seems to cause the trout to take small objects into the mouth, the gustatory sense to decide whether or not they are edible.

9. Before the yolk sac is absorbed the reactions of the young trout are protective, afterward they are exploratory and aggressive.

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THE EARTHWORM AND THE METHOD OF TRIAL

L. H. BITTNER, G. R. JOHNSON, AND H. B. TORREY
Reed College, Portland, Oregon

About ten years ago Jennings attempted to clarify existing conceptions of the behavior of the lower organisms by substituting for what he believed to be an inadequate theory of tropisms a conception that rested on what has come to be known as the "method of trial."

Tropism hypotheses have existed at various times that have differed in various respects. There is no doubt that in one respect or another, some of these hypotheses have been open to just criticism. That the method of trial affords an escape from such criticism, however, is becoming less and less apparent with the passage of time.

Notwithstanding their differences, all tropism hypotheses agree in excluding the conception of orientation by trial reactions. Fundamental to them all is the conception of orientation by means of movements that, with reference to a given source of stimulation, are *predictable* as to direction. However cogent, then, the criticism of a particular variety of tropism hypothesis in other respects, it can hardly affect the fundamental characteristic which they all possess in common.

Some months ago, an analysis of the behavior of *Porcellio scaber* showed that the method of trial was incompetent to interpret the orientation of this organism under photic stimulation.[1] In the present paper we shall consider the orientation, under similar stimulation, of the earthworm (*Allolobophora* sp.), an organism of some complexity of structure, whose behavior has seemed to some observers to lend support to the method of trial. These critics have based their conclusions in part on observations,[2] in part on the identification of "random" with

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[1] Torrey and Hays. The Role of Random Movements in the Orientation of *Porcellio scaber* to Light. *Jour. Animal Behav.*, 1914, **4**, p. 110.

[2] See especially Holmes. The Selection of Random Movements as a Factor in Phototaxis. *Jour. Comp. Neur. Psych.*, 1905, **15**, p. 98.

"trial" movements, a source of confusion that has already been discussed in the paper on *Porcellio* to which we have just referred.

The earthworm comes midway between the sow bug (*Porcellio*) and the leech in the freedom with which it bends its body when reacting to light. It has been shown[3] that the first movements of *Porcellio* after stimulation are away from the source of light. The body moves stiffly as a whole. The photoreceptors are anteriorly placed paired eyes. Holmes cites observations on the leech *Glossosiphonia* that show a wide range of mobility in its response to light, dependent upon its characteristic locomotion. The earthworm does not react stiffly, like *Porcellio*, nor are more than a very few anterior segments concerned in whatever random movements may be observable under photic stimulation. Holmes was led to believe that the method or orientation of the leech is, in principle, the same as that of the earthworm. He calls especial attention to the characteristic waving of the body, preliminary to fixation of the anterior end. Our observations, however, encourage us to place emphasis on the resemblance of the reactions of the earthworm to the behavior rather of *Porcellio* than of *Glossosiphonia*. The random movements of the earthworm have thus appeared to us to be less significant elements in its orientation to light than the observations of Holmes indicated.

It is characteristic of the earthworm when advancing in diffused light, to protrude its anterior end first on one side and then on the other, with successive extensions, in fairly regular alternation. A distinct tendency thus exists for this end, when bent to one side, to bend to the opposite side at the next extension. Mechanical causes, such as tensions in muscles and skin, are probably responsible for it. It is natural to expect evidence of this tendency in experiments on earthworms where relatively low intensities of light are employed unilaterally. Mast, indeed, asserts that in active worms, " the anterior end is simply turned sharply in the direction opposite to that in which it is when it receives the stimulus. . . . Thus it is turned toward the light about as often as from it, regardless of the light intensity."[4] Sluggish individuals, however, reacted quite differently.

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[3] Torrey and Hays, 1914.

[4] Light and the Behavior of Organisms. 1910, p. 200.

From what we judge to have been a neutral position, six sluggish individuals, in one hundred and fifty trials, turned toward the light in but ten of them. In certain other cases there " was no evidence of even the slightest preliminary turning toward the source of light." (P. 201.)

From this evidence ours differs in that our active individuals behaved in the low intensities of light used very much like the sluggish individuals of Mast. Whatever the ultimate significance of this distinction, we have been forced to conclude, as Mast appears to have concluded, that under some conditions, earthworms respond to photic stimulation by orienting reactions that are in no sense trial or random movements. Nevertheless, in our figures, there was unmistakable evidence of that tendency which has been mentioned of the anterior end to swing from side to side. This did not appear, however, in our first series of experiments.

In our first series, sixteen active individuals, taken from darkness, were each subjected to one hundred exposures in quick succession to a very low light intensity. The worm under observation crawled over a moist slate. When, in very weak diffused light, the anterior end was pointed straight forward, the light of a small pocket lamp was flashed upon it from a distance of 50 mm. at an angle of ninety degrees with the body axis. The results are shown in the accompanying table.

From these figures it appears that our earthworms exhibited a marked disposition to react without trial negatively to the light used.

Our second series of observations was taken under somewhat different conditions, and shows very clearly the tendency to which we have alluded above. Each of ten worms was subjected to a total of but thirty trials, in groups of ten. In the first ten trials, the anterior end was bent toward the light at the instant the light was flashed; in the second ten it was in a neutral position, that is, directed forward; in the third ten, it was bent away from the light. Each worm was rested for about seven minutes in darkness after each group of ten trials. A light of slightly greater intensity was used, namely, a 25 w. Mazda lamp, 160 mm. distant, so screened that the ray falling on the worm was about 8 mm. wide. In other respects, the conditions were essentially the same as in the first series.

TABLE 1

	No. of trials	Direction of first movement	
		Toward light	Away from light
Earthworm No. 1	100	18	82
" 2	100	22	78
" 3	100	18	82
" 4	100	28	72
" 5	100	16	84
" 6	100	26	74
" 7	100	34	66
" 8	100	14	86
" 9	100	24	76
" 10	100	28	72
" 11	100	12	88
" 12	100	32	68
" 13	100	20	80
" 14	100	21	79
" 15	100	43	57
" 16	100	34	66
Totals	1600	390	1210
Percentages		24.4%	75.6%

TABLE 2

	No. of trials	Position of anterior end with reference to light					
		Toward		Neutral		Away	
Sense of response		—	+	—	+	—	+
Earthworm No. 1	30	9	1	8	2	9	1
" 2	30	8	2	9	1	9	1
" 3	30	8	2	9	1	7	3
" 4	32	10	0	10	1	8	3
" 5	30	10	0	9	1	7	3
" 6	30	10	0	8	2	9	1
" 7	30	10	0	8	2	8	2
" 8	30	10	0	9	1	9	1
" 9	30	10	0	8	2	10	0
" 10	30	10	0	9	1	9	1
Totals	302	95	5	87	14	85	16
Percentages of first movements away from light		95%		86.13%		83.16%	

Assuming now, the observed tendency of the anterior end to swing in fairly regular alternation from side to side in successive extensions; and assuming, further, the tendency brought out by the figures just given, for the anterior end to swing directly away from the light; one should expect to find the anterior end swinging away from the light *most* frequently, in this second series, when it was turned *toward* the light at the instant the latter was flashed, and *least* frequently when it was turned *away* from the light at the moment of flashing.

This expectation is, in fact, realized in the following figures. The light was flashed on the right of the first seven individuals, on the left of the others.

The third double column of figures is especially significant, as it shows a very marked negative reaction of the worms observed, under the conditions of the experiment, in spite of the conflicting tendency manifested in diffused light to swing the anterior end in the opposite direction.

Holmes has pointed out the danger of failing to notice certain very inconspicuous movements that might be started *toward* the light but not followed up. We have tried to guard against this opportunity for error. At the same time, it may be worth while to remark that a certain degree of extension of the anterior segments appears to be necessary to expose the photoreceptors to effective light intensities. Our figures seem to us to show clearly that photic stimulation, far from inducing random movements, immediately calls forth reactions in a definitely predictable direction. In the face of the facts, a view based upon minute random movements that are not referable to photic stimulation can hardly affect the conclusion that the earthworm must be placed, with *Porcellio*, in that group of organisms whose orientation to light is determined essentially by movements that are predictable as to direction and hence neither random movements nor " trials."

ELIMINATION OF ERRORS IN THE MAZE[1]

HELEN B. HUBBERT

While engaged in a research problem on the learning ability of white rats at different ages, my attention was directed to the question of the elimination of useless movements in the learning of the maze. I decided to test whether or not such eliminations occur progressively, i.e., whether useless movements most closely connected with satisfaction (food) are the first to drop out, while the useless movements most remote from the source of satisfaction (food) persist the longest.

The observations recorded in this paper were made on four groups of rats of different ages during their learning of the Watson maze which, with its camera lucida attachment, is described at length in a previous number of this journal.[2] The process of training and the criteria of learning were the same as those set forth in a previous paper.[3]

As has been pointed out by Watson,[4] according to the pleasure-pain hypothesis, the inference of progressive elimination is plain. Food (the "satisfier") is at the center of the maze. Errors in the alley nearest the food should be the first eliminated; those in the alley next nearest, second, and so on until we reach those in the first alley (the one farthest from the food). Errors in this alley should be the ones last eliminated.

An examination of the plan of the maze will show that in every alley except VI there are three possibilities of error, viz.:

1. Taking the wrong turn at the alley entrance.
2. Going too far in the alley, i.e., past the entrance to the next alley.
3. Taking the correct turn, but returning ("doubling" on the pathway).

In VI the first error is impossible because there is no stop, and either turn leads to the food box. The second error resolves itself into a circling of the food box, which, however, rarely occurs.[5] The third error is the most common one. Starting to the right, the rat retraces its path and goes to the left, or vice versa. Clearly then, the sixth alley is not strictly comparable with the others, and should not be considered. It is therefore set off from the rest in the accompanying tables.

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[1] From the Psychological Laboratory of The Johns Hopkins University.

[2] Watson, J. B. *Journal Animal Behavior*, vol. IV, p. 56.

[3] Hubbert, H. B. *Ibid*, pp. 60-62.

[4] Watson, John B. Behavior. Holt & Co., p. 268.

In the first alley the emotional disturbance of the animal is very great. Whether he will turn to the right or to the left is a matter of pure chance. Watson has shown that the learning of the maze is due largely to the kinaesthetic and organic impulses which cannot begin to play their rôle very effectively until some distance has been run in the maze.[6] As stated above, the start in the first alley is as likely to be in the wrong as in the right direction. If the start is wrong increasing disturbance ensues, and is often carried over into alley II.[7] Aside from this fact, there is the very strong tendency to back-track to the point of entrance (E), which has a different stimulating value from any other part of the maze. It is for these reasons that the first alley as well as the sixth is judged incomparable with the rest and hence is set off from them in the tables.

This leaves for consideration four alleys, II, III, IV and V. Whether the process in question is spoken of as the elimination of errors, the "stamping in" of useful movements and the "stamping out" of useless ones, or simply as the *elimination of alleys* matters little; the facts remain the same. The writer has chosen for convenience to speak of the elimination of superfluous movements in an alley as the elimination of the alley itself, and the results are so tabulated. For example, Rat 4 of Group A made its last error in alley II at the 11th trial, running the alley perfectly in all succeeding trials. The alley is therefore spoken of as eliminated at the 12th trial. Likewise III was eliminated at the 8th trial, IV at the 3rd and V at the 7th trial, no errors being made in those alleys after the 7th, 2nd, and 6th trials respectively. The first column gives the laboratory number of the animal, while column 2 gives the total number of trials the animal required to learn the maze.

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[5] This error seldom occurs, because in passing the entrance to the food box the smell and sight of the food become directive.

[6] Watson, J. B. Kinaesthetic and Organic Sensations—Their Rôle in the Reaction of the White Rat to the Maze. *Psychological Monographs,* Series No. 33.

[7] It is not unlikely that the deviation of the results in alley II from those in III, IV and V may be explained in this way.

In the tables, cases which undoubtedly show uniform progressive elimination, i.e., a 5-4-3-2 order, are doubly starred (**). Cases which might possibly be considered progressive are singly starred (*). Cases clearly not progressive are unmarked.

TABLE I

Group A—25 Days

Rat	Trials	Alleys					
		I	II	III	IV	V	VI
4	18	10	12	8	3	7	3
5	14	7	6	9	9	8	5
6	45	40	32	29	10	37	10
7	66	60	35	43	49	15	1
8	32	25	26	25	17	11	1
9	38	33	22	27	31	24	1
10**	28	21	22	14	8	5	9
11	18	13	12	7	2	10	5
12	32	19	27	16	17	13	1
13	46	39	38	41	13	26	1
14	24	17	17	4	3	12	2
15	34	25	13	11	1	28	4
16	27	17	14	22	5	6	1
17	26	21	13	11	6	7	19
19	40	30	33	34	30	19	5
20**	34	28	28	15	10	9	1
21	36	31	18	25	13	14	2
22	32	27	23	23	18	23	7
23	34	15	19	29	22	23	1
24	24	11	18	9	6	19	10
25	36	19	29	19	19	30	3
Totals.. 21 rats	..	508	457	421	293	346	92
Averages........	..	24	**22**	**20**	**14**	**16**	4

2 cases (**) uniformly progressive or 10%, vs. 90% not progressive.
9 cases where IV and V are eliminated before II and III, or 43%.

DISCUSSION OF THE TABLES

Group A—21 Rats

These rats began the problem when twenty-five days old.

Two of the twenty-one showed uniform progressive elimination, i.e., alley V was eliminated first, alley IV second, alley III third and alley II fourth and last. We find then two cases of uniform progression and nineteen cases clearly not uniformly progressive, i.e., ten per cent progressive versus *ninety per cent* not progressive.

If we now count the cases where IV and V are eliminated before II and III but not in a 5-4-3-2 order, e.g., Rats 4 and 16, we find them to be nine, or forty per cent of the group, which is less than would be expected on a chance basis.

If, however, instead of considering individual cases, we look at the averages, we still find no uniform progression. But here again IV and V are eliminated before II and III.

TABLE II

GROUP B—65 DAYS

Rat	Trials	Alleys					
		I	II	III	IV	V	VI
11	18	13	8	8	9	3	1
12	34	25	28	20	29	25	1
13**	22	9	17	10	3	1	7
14	22	16	15	15	3	7	1
15	28	19	12	22	9	9	3
16	36	30	17	9	31	16	8
17	36	30	25	16	2	26	7
18	66	61	47	24	54	33	5
19	32	26	18	4	2	8	3
20	36	31	20	9	12	9	4
21	46	41	36	37	8	8	6
22	21	15	13	6	6	8	2
23	38	31	22	33	29	26	1
24	38	33	30	15	24	28	1
25	14	9	4	5	9	3	1
27	40	35	29	12	9	25	3
28	20	15	9	10	5	4	1
Totals— 17 rats	..	439	350	255	244	239	55
Averages........	..	26	**21**	**15**	**14**	**14**	3

1 case (**) uniformly progressive or 6%,
vs.
16 cases not uniformly progressive or 94%.
5 cases where IV and V were eliminated before II and III, or 29%.

GROUP B—17 RATS

These animals began the problem when sixty-five days old.

Of the seventeen, one rat showed uniformly progressive elimination while sixteen did not, i.e., six per cent progressive and *ninety-four per cent* not progressive.

TABLE III

Group C—200 Days

Rat	Trials	Alleys					
		I	II	III	IV	V	VI
6	18	12	9	4	7	9	3
7	54	48	46	40	42	35	16
8	44	39	36	33	38	24	21
9	26	13	19	5	21	5	5
10	20	15	5	4	3	4	1
11	20	13	14	4	13	11	11
15	32	27	23	23	7	20	4
17	56	50	35	22	45	19	11
18	79	71	66	30	74	29	12
19	49	44	44	18	9	15	11
20*	27	22	9	8	8	8	8
21	32	26	18	7	13	12	2
23	30	25	17	25	4	22	3
24	30	25	15	13	14	9	1
25	35	26	30	9	30	8	8
27	22	11	17	9	14	6	6
29	104	99	95	74	88	88	5
30*	108	101	98	87	87	38	22
31	64	58	45	55	23	12	1
33	32	27	15	27	24	19	5
34	22	13	15	5	13	17	17
35**	37	30	30	16	13	10	2
36	32	21	8	2	8	15	26
38	14	9	7	4	4	6	1
Totals.. 24 rats	..	825	716	524	602	441	202
Averages........	..	34	**29**	**22**	**25**	**18**	8

1 case (**) or 4% uniformly progressive.
2 cases (*) or 8% doubtful.
21 cases or 88% not progressive.
3 cases or 13% in which IV and V were eliminated before II and III.

Here we find five rats eliminating IV and V before II and III, or twenty-nine per cent.

The averages show possible uniform progression, although the values for III, IV and V are too nearly identical to warrant such an interpretation.

Group C—24 Rats

These rats began the problem when two hundred days old.

Of them, one rat showed uniform progressive elimination, two possible progressive elimination, while twenty-one did not show such progression. Stated in percentages, four per cent were progressive, eight per cent possibly progressive and *eighty-eight per cent* non-progressive.

Here we find only three cases or thirteen per cent where IV and V were eliminated before II and III. The averages showed no progression; IV and V were not even eliminated before II and III.

TABLE IV

GROUP D—300 DAYS

Rat	Trials	Alleys					
		I	II	III	IV	V	VI
15	78	67	72	35	39	35	3
16	20	14	11	9	10	8	1
17	48	43	28	15	36	14	3
18	40	31	35	9	32	34	7
19	14	9	9	4	9	5	1
20**	58	45	52	35	28	27	5
21	30	25	12	6	9	9	13
22	82	77	67	47	64	66	66
24	42	37	33	26	25	28	34
25	54	49	27	37	49	49	37
26*	19	14	10	7	7	7	4
27*	70	65	61	24	24	22	24
28	38	33	24	20	6	24	5
30	27	22	16	15	8	13	6
31	84	78	32	53	72	69	69
33	16	11	8	5	2	11	3
34	66	60	60	25	26	48	27
35	38	32	20	19	20	17	9
36	26	15	15	5	15	11	21
37	44	39	29	17	13	30	5
38**	34	28	23	15	8	6	1
39**	35	5	29	23	21	11	4
Totals.. 22 rats	..	799	673	451	523	544	128
Averages........	..	32	**30**	**20**	**24**	**25**	16

3 cases (**) or 14% uniformly progressive.
2 cases (*) or 9% doubtful.
17 cases or 77% not progressive.
3 cases or 14% where IV and V were eliminated before II and III.

GROUP D—22 RATS

These rats began the problem when three hundred days old. Three of the twenty showed uniformly progressive elimination, two showed possible progressive elimination, while seventeen did not show uniform elimination, i.e., fourteen per cent were progressive, nine per cent possibly progressive and *seventy-seven per cent* not progressive.

There were three cases where IV and V were eliminated before II and III or fourteen per cent.

The averages do not show progressive elimination nor were IV and V eliminated before II and III.

We do find, however, in every group that alley II is nearly always the last to be eliminated. A possible explanation of this has already been offered on page 67. The results in alleys III, IV and V are so nearly identical that the three may be considered as eliminated at practically the same time.

From these experiments it seems fairly probable that the rapidity with which a given co-ordination in a complex habit is formed is not proportional to the distance from the point at which the co-ordination takes place to the point at which food is to be obtained.

NOTES

NOTE ON THE REACTION OF THE HOUSE-FLY TO AIR CURRENTS

F. ALEX. McDERMOTT
Mellon Institute, University of Pittsburgh, Pittsburgh, Pa.

While making some experiments on the drying of certain vegetable materials in a current of air, the following observation was made, which may be of interest.

The material had a strong attraction for flies, of which there were several in the room, and as their presence did not interfere with the work, no precautions were taken to screen them off. The apparatus consisted of a flat bottomed aluminum dish, 20 cm. in diameter, with vertical sides 8 cm. high; into this dish was blown a current of air having a volume of about one-fourth to one-half cubic meter per minute, by means of an electric hair-drier (speed equals about 100 meters per minute). The air was slightly heated, showing 29 to 30° C., when the room temperature was 26 to 27° C. The air current struck the center of the bottom of the dish at an angle of 60°, passing over two 5 cm. aluminum dishes, in which the material being dried was contained. Flies alighting on the material in these dishes soon turned toward the direction from which the air was coming, walked down over the edge of the dish, on to the bottom of the large dish, and toward the point where the air current struck the bottom, usually stopping two or three centimeters from the center; here they would remain, with their axes parallel to the direction of the air current, and their heads facing to windward for half an hour or longer, if not disturbed. They appeared to be pressed down against the bottom of the dish by the force of the air current, quivering slightly with variations in the pressure, and they were observed not to be feeding. New comers, alighting in any other than the position above described, moved about in short jerks, until they had assumed this position. Sometimes chains of two or three, immediately back of one another, would be formed, with spaces of only a few millimeters between them, though more usually they placed themselves so as to have the head in the direct air current. Those in other portions of the dish assumed positions with the axis parallel to the stream lines, with the head to windward, even though they happened to be in a slight eddy current. A thermometer was placed in the dish, inclined toward the fan; a few insects climbed up this toward the fan, but the current appeared to be too strong for them. Increasing the temperature of the air current to 40° C. caused scattering and finally flight, though the flies seemed reluctant to go, rather attempting to back away slowly, before taking to flight. Sudden stopping of the current of air caused immediate dispersal. While most of the insects took to flight at once on being disturbed mechanically, as with the bulb of the thermometer, a few of them would allow themselves to be pushed about and even pressed down tightly on the bottom of the dish, with the thermometer bulb, without taking flight. The writer believes that the observation has been made that flies lighting on moving vehicles usually turn with the axis parallel to the direction of motion, and with the head forward, but he is not aware of any observation of the kind here recorded. Unfortunately, means were not at hand to try the effect of wider and lower variations of the temperature of the air.

FINANCIAL STATEMENT

For the information of its subscribers, contributors, and benefactors, the Journal of Animal Behavior proposes hereafter to publish, in the first number of each volume, a brief statement of its financial condition.

Financial Statement for the Journal of Animal Behavior, December 30, 1913 to December 1, 1914 (Volume 4)

RECEIPTS

Balance from 1913	$154.67	
Receipts from sales of complete volumes and odd numbers	786.30	
Receipts from advertising	50.00	
Gifts and contributions toward the cost of illustrations and tabular material	222.13	
Interest	27.26	$1,240.36

EXPENDITURES

Cost of manufacturing and distributing volume 4 (this does not include cost of paper, paid in 1913)	$868.48	
Office expenses, including postal and express items	180.00	1,048.48
Balance on hand		$191.88

JOURNAL OF ANIMAL BEHAVIOR

Vol. 5 MARCH-APRIL 1915 No. 2

A STUDY OF THE BEHAVIOR OF THE CROW *CORVUS AMERICANUS* AUD. BY THE MULTIPLE CHOICE METHOD

CHARLES A. COBURN AND ROBERT M. YERKES[1]
The Harvard Psychological Laboratory and the Franklin Field-Station

We have previously reported in this Journal[2] observations on the behavior of crows in certain forms of visual discrimination. The subjects of that investigation were transferred from the Franklin Field-Station in September, 1913, to the Laboratory of Animal Psychology in Cambridge, and were there kept until June, 1914, in a cage approximately six feet in its several dimensions. Despite their close confinement and the lack of an out-of-door fly, the birds continued in excellent health and proved themselves able to withstand wholly satisfactorily the conditions of laboratory life. When returned to the Field-Station, they were considerably less tame than during the previous summer. For this reason they were not used further for experimental purposes, but were kept for general observations. Young crows were captured for the experiments which are reported in this paper.

Instead of following up the study of visual discrimination, we devoted our attention, during the summer of 1914, to an attempt to analzye ideational and allied forms of behavior in the crow by means of the Yerkes multiple choice method, and to the accumulation of additional facts concerning the natural history, instincts, and general habits of the birds.

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[1] The observations reported were made chiefly by Mr. Coburn and the paper was written by Mr. Yerkes.

[2] Coburn, C. A. The behavior of the crow *Corvus Americanus*, Aud. *Journal of Animal Behavior*, 1914, **4**, 185-201.

On June 7th, 1914, three young crows were captured near the Station. These birds were about ready to leave the nest. One, indeed, was taken from a limb beside the nest. This individual from the first exhibited fear and was so troublesome that after two days it was discarded and the remaining two birds were kept for observation. They were placed in a box which was frequently passed by human beings, and were several times a day fed by hand, being allowed to come out of the box at will and become thoroughly accustomed to the experimenters. From the time of capture they were perfectly tame, ate readily, and the characteristic fear reactions never appeared. When taken from the nest, they were probably at least six weeks old.

Throughout this report, these birds will be referred to as number 3 and number 4. Number 3 was from the first the larger of the two and the less timid. It, during the several months of observation, always came to us, perching on arms, shoulder, or head, as it had opportunity, and showing a friendly interest which was apparently somewhat independent of its desire for food. It evidently liked to be petted. Our assumption is that this bird is a male.[3] Number 4, by contrast, was smaller, shyer, more wary, and after a few weeks ceased to come to either of us, except as drawn by hunger, and even then it often hesitated to perch upon the hand or arm. In all probability, it is a female. It has eaten less than number 3, and has been considerably more difficult to experiment with. Usually, in the course of an experiment, if the birds were in competition, number 4 would stand aside for number 3.

Our additional experience with crows during the present season but emphasizes our conviction that they are among the most interesting of birds, and that their behavior is in every respect worthy of careful analytic study. With respect to what we shall term "ideational behavior," they have fallen short of our expectations, for in the light of their varied interests, ingenuity, curiosity, ceaseless activity, and apparent insight into simple situations, we had assumed that they possess an intelligence equal to that of many of the more intelligent mammals. The experiments now to be reported were conducted for the special purpose of obtaining definite and reliable information concerning the nature and limitations of their ability to adjust themselves to certain fairly simple, although novel, situations.

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[3] Since this was written, dissection has definitely established our surmise in the case of both birds.

We sought to make our measurements of intelligence by a method recently devised at the Psychopathic Hospital, Boston, by R. M. Yerkes, for the comparative study of ideational and allied forms of behavior in man and other animals. This method has been named the soluble-problem multiple-choice method. It was devised primarily for the purpose of enabling the comparative psychologist to present to any human or infra-human subject, no matter what the age, degree of intelligence, or condition of normality or abnormality, a series of situations increasing in complexity from an extremely simple one to one so intricate that even the most intelligent human subject might spend hours or days in adjusting himself to it. By means of this multiple choice method, it is hoped and confidently expected that the materials of comparative psychology may be rapidly increased and the analyses of animal behavior be made invaluable to the psychopathologist.

A general description of the method should preface this account of the special form in which it was applied to the crow, inasmuch as only a very brief account of it has been published.[4]

In brief, the essentials of the method are these. A series of reaction mechanisms, appropriate to the subject, are presented. From this series one mechanism must be selected which, when properly approached, will yield the subject the satisfaction of success and, possibly, the reward of food. With each presentation of the reaction mechanisms, they are varied in number and in position. The subject is therefore forced to select the proper mechanism on the basis of some particular relationship of that mechanism to its fellows, this relationship having been determined upon in advance by the experimenter. It may be, for example, such a simple relation as first at the left of the series as the subject approaches, or first at the right of the series, or second at the left, or alternately the first at the left and the first at the right, or the middle of the series. Imagine, then a series of piano keys which may be presented to a human subject. They may vary in number from two to twelve (this was the original form of apparatus). Some one key, in any group of keys presented, when pressed will cause a bell to ring, thus indicating success. Without other aid than his own observation, the subject is expected, from repeated presentations of the keys, to discover the essential relation and to acquire the ability to select the right key with certainty.

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[4] Yerkes, Robert M. The study of human behavior. *Science*, 1914, 39, 625-633. In this paper the writer describes his method in contrast with the Hamilton quadruple choice method.

This method has the advantage of enabling the experimenter to present increasingly difficult problems to his subjects. It has further the advantage of enabling him conveniently to record the essential features of reaction, and later to analyze the reactions at his leisure. But most important of all, it yields strictly comparable results when applied to widely differing organisms. Naturally, although the same problems may be presented to diverse types of organism, the reaction mechanisms must be suited to the subject in question.

Without further general comment or discussion of the multiple choice method, we shall describe the form of apparatus and procedure employed with the crow.

APPARATUS AND METHOD

In the accompanying plate, designated as figure 1, and in the ground plan of the observation-room and apparatus, shown in figure 2, the general experimental situation is represented. Figure 1 shows in the background the building which was used both as a shelter for the crows and as an observation place for the experimenter. To this building is attached a fly which appears in C, D, and E of Figure 1. In figure 2, the ground plan of the building, are seen the experimenter's room, A, and the crow room, B, the latter containing a perch, P. All coarsely dotted lines in this figure indicate walls or partitions made of poultry wire. The large fly was, for the purposes of our experiment, divided into two parts by a wire partition. In the smaller of these portions, shown at the right of figure 2, the multiple choice apparatus was located. The crows could enter this portion of the fly only at the will of the experimenter, whereas they were allowed the freedom of the larger portion, which we have labelled C. As figure 2 is drawn to scale (one inch to forty-eight) it is unnecessary to give the measurements of the

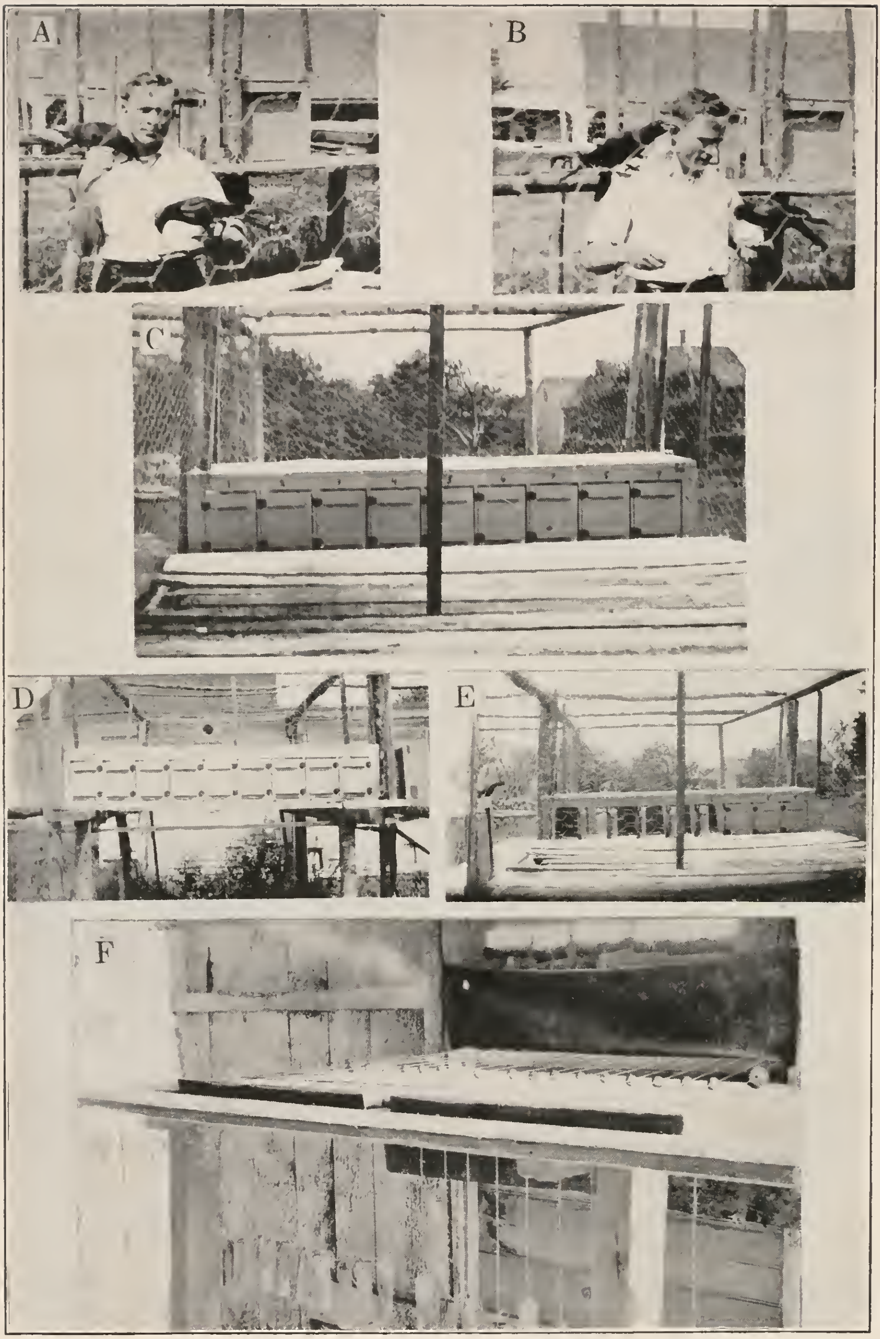

FIGURE 1. Views of crows and apparatus for multiple choice experiments. A and B, crows, number 3, ♂, on shoulder of experimenter, and number 4, ♀, on arm; C, the multiple choice box seen from the observer's room and from the direction of approach by the crow, the compartments are numbered 1 to 9 and below each number is an entrance door. D, the same seen from the opposite side or rear, with the nine exit doors closed. E, the box seen from the observer's room, with entrance doors 1 to 6 and exit door 2 open. At the extreme left, above the entrance door to the experiment compartment of the fly, one of the crows is visible. F, the observer's table, showing curtain before window (partially drawn aside to admit light for the camera) and the weighted cords with pull buttons for opening and closing doors.

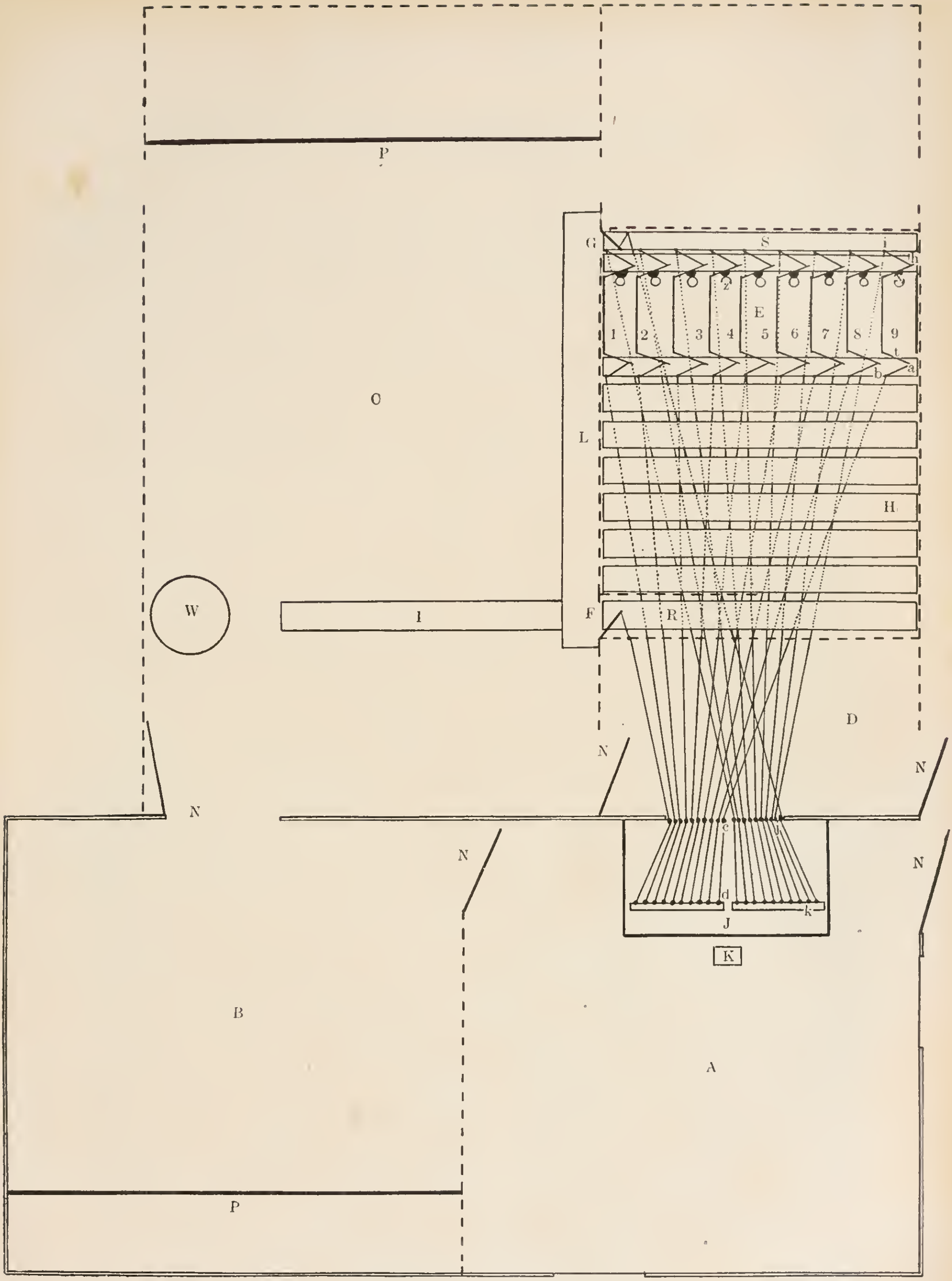

FIGURE 2. Ground plan of crow house, fly, and apparatus. Scale, $\frac{1}{48}$. A, observation room; B, bird room; C, main portion of fly; D, passageway for experimenter; E, multiple choice box; F, entrance door between main fly, C, and alley to reaction chamber, H; G, exit door between alley S and main fly; H, reaction chamber, the floor boards of which are separated somewhat; I, L, approaches to the doors F and G; J, observer's table and key-board; K, observer's stool; N, doors for experimenter's use; P, perches; R, alley leading to middle of reaction chamber H; S, alley leading from exits to main fly; W, water tub for crows.

Numerals 1 to 9, compartments of multiple choice box; a, attachment of cord to entrance door, t, of compartment 9; b, screw eye for cord; c, screw eye at entrance to observer's room; d, wooden button on cord; under d is a small brass pulley for cord; h, i, j, k, indicate course of cord from exit door of compartment 9 to key-board; x, metal cover for food receptacle of compartment 9; z, food receptacle of compartment 4.

building and fly. We shall give a more detailed description of the experimental device.

The latter is shown fairly well from different points of view in the parts of figure 1. Figure 1 C is a view of the multiple choice box from the front, that is, the side of approach by the subject. All the entrance doors are closed. Figure 1 E shows the apparatus from the same point of view, with the entrance doors 1 to 6 and the exit door 2 open. Figure 1 D, instead, shows the apparatus from the opposite side, with the several exit doors closed.

By referring now to both figures 1 and 2, we should be able to obtain a clear idea of the construction of the experimental mechanism and its use.

The multiple choice box, as we shall call it, appears in ground plan as E of figure 2. It is divided into nine like compartments, each with a door at both ends, opening outward. The outside measurements of the multiple choice box are 81 inches long by 20 inches wide by 15 inches high. The frame of the box is made of 2 by 2 inch stock, and the floor, ends, partitions, and doors, of half-inch stock. The top, which is hinged for convenience of access, consists of wire netting, 1¼ inch mesh, on a wooden frame. On the inside, each of the nine compartments is 19 inches long by 8 inches wide by 13 inches high. The entrance and exit doors are 9½ inches high by 7⅝ inches wide. All of the doors are mounted with spring hinges which hold them shut. On the lower inner edge of each exit door is a piece of tin (x) which, when the door is closed, projects 2 inches into the compartment and covers a hole (z) in the floor of the compartment 1½ inches in diameter by ⅞ inches deep. These metal covers, as well as the holes, are represented in the ground plan of the apparatus, figure 1, x and z. The use of these holes is to contain food which serves as a reward for the bird when the exit doors are opened.

The system of entrance and exit doors, nine of each, and also the main entrance door, labelled F in figure 2, and shown in the extreme lower left corner of figure 1 E, and the main exit door, labelled G in figure 2, and shown at the right end of the multiple choice box in figure 1 D, are controlled from the experiment room A by a system of cords passing through screw eyes and pulleys.

These cords are indicated by dotted lines where they pass under the floor of the multiple choice box or under the boards which serve as an approach to the box: Elsewhere they appear as solid lines. The arrangement of the cord-system within the experiment room is rather unsatisfactorily shown in figure 1 F.

On a table, J, before which the observer sits on the stool, K, are two groups of cords, each with a wooden button attached in a convenient position. The group at the experimenter's left consists of the cords connected with the ten entrance doors, and the group at the right, similarly, of those connected with the ten exit doors.

We may now trace the course of the cords from the doors of compartment 9. A cord is fastened at a to the lower outer corner of the entrance door t. It thence passes through the screw eye b in the edge of the approach board. From this point it extends, under the interrupted floor of the reaction chamber H, to a screw eye, c, in a block across the aperture leading to the experiment room. Thence the cord passes over a small brass pulley at d and through a hole in the table J. (In figure 2 the pulley is hidden by the wooden button on cord.) It is kept taut by a lead weight under J. Similarly, the cord for the exit door of compartment 9 is attached to the lower outer corner of the door at h, passes through the screw eyes, i and j, to the pulley k, and is kept taut by a leaden weight. The cords for the main entrance and exit doors, F and G, run to the extreme left and right respectively of the experimenter's table.

The experimenter operates a door by grasping the wooden button shown on each cord in figure 1 F and pulling it toward him. When he has pulled as far as the button will come, the door to which the cord is attached stands wide open, and the leaden weight under the table serves to hold it in this position as long as the experimenter desires. When he wishes it closed, he simply pushes the button back to its former position, and the strength of the spring hinges suffices to overcome the pull of the weight.

In order that the bird should not see and be influenced by the movements of the experimenter, a black curtain was hung before the opening into the experiment room, and through small holes cut in it, the experimenter was able to observe the movements of his subject. At no time during the investigation did the crows give evidence of noticing the experimenter when they were reacting.

The remaining features of the apparatus will be mentioned in connection with the following brief description of the experimental procedure. In preparation for a series of trials, the experimenter opens each of the exit doors and places in each food container a small bit of milk-soaked bread. He then closes the exit doors, thus covering the food, and takes his place at K. He next opens a group of entrance doors. Let us suppose, as is shown in figure 1 E, that the doors numbered 1 to 6 are opened, and, further, that the compartment which may be designated as the correct one is the first at the subject's left, that is number 1. Having made these preparations, the experimenter, by means of the proper cord, opens the main entrance door F, and the bird, either by walking up the approach board I or by alighting on the approach board L, on a level with the entrance door, is immediately able to enter the reaction chamber H, by way of the alley R. By two wire partitions which appear as dotted lines in figure 2, it is forced to walk straight ahead until it reaches the center of compartment H. It may then face and, if it so chooses, directly approach the central compartment of the multiple choice box. But under the circumstances, with entrance doors 1 to 6 open, it would naturally swerve toward the left.

In case it enters compartment 1, the experimenter quickly and noiselessly closes the entrance door after it, by releasing the appropriate cord, and immediately thereafter, opens the exit door of the compartment by pulling on the appropriate cord. He, thus, with one hand prevents the retreat of the bird from the compartment and with the other uncovers the food, so that the bird may obtain the reward for a correct reaction. As soon as the food has been swallowed, the crow steps out of the compartment, the exit door is closed by the experimenter, and the bird either immediately, or at the experimenter's pleasure, is allowed to return to the fly C by way of the main exit door G.

If, instead of choosing the right compartment, the crow enters some other one, the procedure is different. Immediately upon its entrance, the experimenter closes the entrance door. He then, with a stop-watch, measures a definite period during which the bird is confined in the compartment. This period was varied during our experiments from 15 to 60 seconds, in an attempt to discover the most satisfactory length of confinement. At the proper moment, the experimenter opens the entrance door and the crow is allowed to retrace its steps. It may then immediately make another choice. But not until it enters the right compartment, is it awarded with food and allowed to return to the fly. Thus punishment for incorrect choices is combined with reward for correct choices.

In further description of the apparatus, it should be said that wire partitions at each side of the multiple choice box, and extending from the lid of the same to the roof of the fly, prevented the crow from walking or flying over the box, while boards both in front of the box and behind it and on a level with its floor form a floor which prevented the bird from getting under the box. The only possible course for the subject from main entrance to main exit door is by way of one of the compartments.

Experience shortly indicated that the crows could be used most satisfactorily if given their trials alternately, and the method finally settled upon was that of admitting one crow to the apparatus, allowing it to make its choice, and then holding it in the passageway beyond the exit doors until the other crow had passed through the main entrance door into the reaction chamber. Thus, as one subject emerged from a compartment of the box E, the other bird entered the reaction chamber. When, as sometimes happened, the one or the other bird failed to respond immediately and appropriately and both were in the fly, it was fairly easy for the experimenter to admit the proper bird by carefully manipulating the entrance door.

PRELIMINARY TRAINING

The crows obtained almost all of their food in the multiple choice box. In order that they should work steadily and industriously, it was necessary to have the pieces of bread or mouse meat, which was sometimes used instead of bread, very small. It proved possible to obtain as many as twenty reactions per day from each bird, in series usually of five each.

We shall now consider the course of experimentation and its results. One June 21st, the crows having attained ability to feed themselves, preliminary training was undertaken, and from that time they were fed in the multiple choice box. They exhibited no fear, rapidly became familiar with the apparatus, and acquired skill in making the trip from the main fly, through the experiment compartment, back to the fly. For several days, both the entrance and the exit doors of the compartments were kept open. Then the situation was changed by the closing of the exit doors, and the crows were trained to enter a compartment and wait for their food.

On June 27th, the first series of trials worthy of special mention was given. The apparatus was in perfect working condition. Food was placed *on the floors* of the several compartments; the exit doors were closed and the entrance doors were open. The main entrance door was opened, and both birds were allowed to enter the reaction chamber and go to the compartments for food. As they entered the compartments, the exit doors were opened and the entrance doors closed. Thus, by a series of trials they were habituated to the opening and closing of the doors and were taught to make the circuit promptly from the main fly back to the same by way of the multiple choice box.

On the following day, June 28th, the food was placed in the food containers and the exit doors were closed. Number 3 entered the compartments rapidly and made the circuit usually without delay, but number 4 at first refused to enter the compartments. Within two days, it, however, was readily entering, in its search for food.

On June 28th, only three or four of the entrance doors to the compartments were opened at any one time. In the previous preliminary training all of the doors had been opened. Neither bird showed any marked preference for a particular compartment in the multiple choice box.

On June 30th, the method was tried of confining one of the crows in the crow room B, of figure 2, while the other was given its trials. Later in the day, the birds were given another series of trials alternately, the one being kept in the exit alley as described on page 83, until the other had entered the reaction chamber. This method proved satisfactory and was later employed to the exclusion of the former.

Up to this point, the two subjects adapted themselves to the different situations with almost equal rapidity. Number 4 was somewhat less willing to try new things than number 3, and seemed to be hampered by its shyness.

A significant incident is the following. In one of the trials› number 4 accidentally fell through a five inch space which had been left before the entrance doors in order that the crow should not too closely approach a compartment unless it intended to enter it. The bird fell to the ground beneath the apparatus, finding there some pieces of bread which had been dropped earlier in the day. Naturally enough, it ate them before it could be induced to return to the fly. Ever thereafter, until this crack had been closed, this bird, as it approached the compartments, would look through the crack to the ground. Several times it flew down in search of food.

RESULTS, PROBLEM 1

With the final series of trials given on June 30th, regular experiments were initiated. *The problem which the birds were required to solve was that of learning to select the first open door at the right.*

Ten settings as we shall call them, were chosen by the experimenters. These are given below, numbered 1 to 10. After each number appears the series of open doors; in the next column, the total number of doors open; and finally in the last column, the number of the right compartment in which the reward of food might be obtained.

Problem 1. First door at the subject's right to be chosen

Settings	Doors open	No. of doors open	No. of right door
1	7.8.9	3	9
2	2.3.4	3	4
3	3.4.5.6.7	5	7
4	1.2	2	2
5	2.3.4.5.6	5	6
6	6.7.8	3	8
7	3.4.5	3	5
8	2.3.4.5.6	5	6
9	1.2.3	3	3
10	7.8.9	3	9

In this series of ten settings, a total of thirty-five doors were open, of which number, ten were of course "right doors." Consequently, the chance of a selection of the right door, without previous experience or trial, is one to two and one-half.

In general, it was the purpose of the experimenter, as far as possible, to follow through this series of settings from 1 to 10, and then to return to the beginning and repeat the series. No matter how many trials in succession could be given, the experiments were resumed at the point of interruption of the regular series of settings. Thus, if five trials were given, beginning with setting 1 and extending through setting 5, the next series would begin with setting 6 and continue through setting 10.

As a matter of convenience, it was also decided to have the two crows work on different settings. For example, while crow 3 was presented with the settings 1 to 5, crow 4 would be presented with settings 6 to 10. This enabled the experimenter to avoid the necessity of refilling the food containers after each trial, and it also prevented the crows from developing the tendency to follow one another by sensory cues.

After a very few days of experimentation, both birds reacted with remarkable alacrity and facility. They were, as a rule, prompt to enter the reaction area and almost as prompt to leave the exit area.

In the initial regular experiments, thirty seconds confinement in the wrong compartment was used as punishment for mistakes. But it shortly appeared that this was too long an interval, for the birds hesitated to enter any of the compartments after a half minute confinement in one of them. It was therefore decided to use the period of fifteen seconds as punishment for incorrect choices. Especially during the early experiments, the crows often exhibited considerable fear and excitement when shut in the small compartments. This diminished toward the end of our work, and it was then possible to confine them for a half minute or even a minute without causing disturbing excitement.

The experimenter kept, as a matter of routine, a record of the time from admission to the reaction chamber to entrance into the right compartment. There is no special reason to consider these records significant, and we shall omit them from this report.

Careful record was also kept of the chief features of the behavior of the bird during this interval. The simple system of symbols, which appears below, was adopted for this purpose.

○, to center of the reaction area
┌, to left hand far corner of the area
┐, to right hand far corner of the area
└, to left hand near corner of the area
┘, to right hand near corner of the area
⊔, to center of the near side of the area
⊏, to center of the left side of the area
⊐, to center of the right side of the area

If the crow merely looked into one of the compartments without entering, the number of the compartment was recorded.

If it, instead, entered a compartment, the number was underscored. In case the compartment entered happened to be a "wrong one", the time of entrance was placed in parenthesis immediately after the number of the compartment. When the time exceeded a minute, the number indicating the minutes was placed in a circle. For less frequent forms of behavior, other provisions were found convenient, and by the use of symbols and other abbreviations it was found easy to obtain a fairly complete description of the subject's behavior.

To illustrate the use of the above symbols, the following record of a trial (trial 32 of number 3 on July 23), setting 1.**2**, is presented.

> 4, 3, └, ┘, └, ①, └, o, └, 1, └, ②, (trying to get out of area),
> └, ┘, ③, ┘, └, ┘, (pkd. at hole in floor), ④, 1, o, └, 9, ⑤, ⑥,
> ⑦, o, ┌, └, 2, ⑧, 1, **2**, 8′ 13″.

In this trial, crow number 3 did not enter the wrong compartment at all. The time between the fifth and the seventh minutes was spent before compartment 9.

It was decided by the experimenters that when a crow had made ten correct choices in succession, its training should be considered complete, or in other words, it should be said to have solved the problem.

In the case of the problem in question, crow number 3 at the end of thirty-two trials had entered the right compartment twelve times in succession, but in several of these trials it had been aided by the experimenter, who moved the exit door slightly in order to attract the attention of the bird after it had for several minutes refused to enter any compartment.

In the accompanying table 1, a summary of the trials for each of the birds in problem 1 appears. At the head of the several columns are the settings numbered 1 to 10, with the right number in bold faced type. In the first column at the left, under each of the several settings, appears the number of the trial and the series of compartments entered. Thus, for example, referring to the results for crow number 3, in trial number 5, which was the first trial in the regular series, the bird entered compartment 8 and then compartment 9. In trial 6, it immediately entered compartment 4, the right one. The letter a, following a number, indicates that the bird was aided in its choice by the experimenter.

It is possible by careful study of this and succeeding tables to discover the reactive tendencies of the organism, and to note both the appearance and the disappearance of the same.

Problem 1, the first door at the right, proved a very easy one for both crows. It was mastered by number 3 after fifty-five trials, and by number 4 after fifty-one trials.

Table 2 presents the results of the various series of trials, ranging in number from three to five for each subject. The number of successes and failures in each series and the ratio of successes to failures for each day appear. The letter R in this table indicates correct first choices, the letter W, incorrect first choices. The table has to do only with first choices.

In contrast with the above results in problem 1, the first door at the right, we present in table 3 a summary of the results for problem 1a, the first door at the left, the trials for which were given not immediately after those just described but at the end of the season, and after the crows had for several weeks worked on problem 2, the second door at the left. Naturally the influence of their training to go to the second door retarded the formation of the habit of choosing the first door at the left. For the satisfactory solution of the problem, one hundred trials were required by each bird. Doubtless a change of experimenters after trial 75 somewhat delayed progress. The results which appear in tables 3 and 4 demand no further comment.

Recurring now to problem 1, it is obvious that from the human point of view this is a very simple problem. The crows solved it readily, but in the course of their work they frequently experienced discouragement and were aided in a considerable number of their early trials by the experimenter. Doubtless our results would be more significant had this aid been withheld, but at the outset of our work we hesitated to run the risk of spoiling our subjects by over-discouraging them. In problem 1a, no aid was needed.

Varied reactive tendencies do not appear in connection with this problem. Very few wrong choices were made. Consequently, all that can be gleaned from the results is a general knowledge of the behavior of the crow in the face of a certain fairly simple experimental situation.

TABLE 1

Results For Crow Number 3 in Problem 1

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	7.8.**9**	T.	2.3.**4**	T.	3.4.5.6.**7**	T.	1.**2**	T.	2.3.4.5.**6**	T.	6.7.**8**	T.	3.4.**5**	T.	2.3.4.5.**6**	T.	1.2.**3**	T.	7.8.**9**
5*	8.9	6	4	7	7a	8	2												
						9	2	10	4.6	11	8	12	5	13	6	14	3	15	8.9a
16	9a	17	4a	18	7a	19	2a	20	**	21	8a	22	5a	23	6				
														24	6	25	3	26	9
27	9	28	4																
29	9	30	4a	31	7	32	2	33	5.6	34	8	35	4.5	36	6	37	3	38	9
39	8.9	40	4	41	7														
42	8.9	43	4	44	6.7	45	1.2	46	6	47	8	48	5	49	6	50	3	51	9
52	9	53	4	54	7	55	2	56	6										

* Records for first four trials:
(1) 1.2.**3** (2) 8.**9** (3) 6.7.**8** (4) 3.4.5.6.**7**
3 9 6.8a 3.7a

** Blanks indicate failures to choose.

RESULTS FOR CROW NUMBER 4 IN PROBLEM 1

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	7.8.**9**	T.	2.3.**4**	T.	3.4.5.6.**7**	T.	1.**2**	T.	2.3.4.5.**6**	T.	6.7.**8**	T.	3.4.**5**	T.	2.3.4.5.**6**	T.	1.2.**3**	T.	7.8.**9**
*										5	6.7.8a	6	5	7 2	.6a	8	—		
																9	3	10	9
11	8.9a	12	4a	13	7	14	2												
						15	2	16	6	17	8	18	5	19	4.2.6	20	3	21	9
22	8.9	23	4	24	4.7	25	2	26	6	27	7.8	28	5						
										29	6.8	30	5	31	6	32	3	33	9
34	9	35	3.4	36	7			37	6										
38	8.8.7.9											39	3.5	40	6	41	3		
										42	8	43	5	44	6	45	3	46	9
47	9	48	4	49	7	50	2	51	6										

* Records for first four trials:
(1) 1.2.**3** / 3 (2) 8.**9** / 9 (3) 3.4.5.6.**7** / 5.5.7a (4) 2.3.4.5.**6** / 6

TABLE 2

Daily Series and Averages with Ratios of Correct to Incorrect First Choices

Problem 1

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 30	4	2	2	2	2	1: 1	June 30	4	3	1	3	1	1:.33
July 1	3	2	1				July 1	3	1	2			
" "	1	1	0				" "	1	0	1			
" "	3	2	1				" "	3	2	1			
" "	3	3	0	8	2	1:.25	" "	3	2	1	5	5	1: 1
" 2	2	0	2				" 2	2	2	0			
" "	4	0	4	0	6	0: 6	" "	4	3	1	5	1	1:.20
" 3	3	1	2				" 3	3	2	1			
" "	5	5	0				" "	5	3	2			
" "	4	3	1				" "	4	3	1			
" "	4	2	2	11	5	1:.45	" "	4	2	2	10	6	1:.60
" 4	5	4	1				" 4	5	4	1			
" "	5	3	2				" "	5	5	0			
" "	5	5	0	12	3	1:.25	" "	5	5	0	14	1	1:.07
" 5	5	5	0	5	0	1: 0							

TABLE 3

Results for Crow Number 3 in Problem 1a

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	**1**.2.3	T.	**7**.8.9	T.	**3**.4.5.6.7	T.	**8**.9	T.	**2**.3.4.5.6	T.	**6**.7.8	T.	**5**.6.7	T.	**4**.5.6.7.8	T.	**7**.8.9	T.	**1**.2.3
1	3.2.3.1	2	8.9.8.7	3	3	4	9.8	5	3.2	6	6	7	5	8	5.4	9	8.8.8.8.7	10	3.2.3.2 2.2.1
11	3.2.1	12	8.7	13	3	14	9.8	15	2	16	6	17	5	18	4	19	8.8.7	20	3.2.2.2.1
21	3.2.2.1	22	8.7	23	3	24	9.9.8	25	2	26	6	27	5	28	4	29	8.8.8.7	30	2.1
31	3.2.3.1	32	8.7	33	3	34	8	35	2	36	6	37	5	38	4	39	8.8.8.8 8.8.7	40	2.3.3.1
41	2.2.3.1	42	7	43	3	44	8	45	2	46	6	47	5	48	4	49	8.7	50	2.3.1
51	2.1	52	8.8.7	53	3	54	8	55	2	56	6	57	5	58	4	59	7	60	2.1
61	2.3.2.2 2.2.1	62	7	63	3	64	8	65	2	66	6	67	5	68	4	69	7	70	2.1
71	2.1 **7**.8.9	72	7. **6**.7.8.9	73	3 **2**.3.4.5 6.7	74	8 **4**.5.6.7	75	2* **1**.2.3.4 5.6		**5**.6.7.8 9		**1**.2.3.4 5.6.7.8.9		**3**.4.5.6 7.8		**7**.8.9		**2**.3.4.5
76	8.7	77	8.6	78	2	79	7.4	80	1	81	5	82	2.1	83	3	84	8.7	85	3.2
86	7	89	6									90	1	87	3			88	2
91	1	92	7	93	3	94	8	95	2	96	6	97	5	98	4	99	7	100	1

RESULTS FOR CROW NUMBER 4 IN PROBLEM 1A

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	**1**.2.3	T.	**7**.8.9	T.	**3**.4 5 6.7	T.	**8**.9	T.	**2**.3.4.5.6	T.	**6**.7.8	T.	**5**.6.7	T.	**4**.5.6.7.8	T.	**7**.8.9	T.	**1**.2.3
										1	7.8.6	2	5	3	8.7.6.6 7.5.6.7.5 7.6.8.4	4	7	5	3.2.3.1
6	3.2.1	7	7	8	3	9	8	10	2	11	6	12	5	13	4	14	7	15	3.2.1
16	2.3.1	17	8.8.7	18	5.3	19	8	20	4.2	21	7.8.6	22	5	23	4	24	7	25	3.2.3.1
26	3.2.3.1	27	7	28	3	29	8	30	4.5.6.3.2	31	6	32	5	33	4	34	7	35	3.2.2.1
36	3.1	37	7	38	4.5 3.	39	8	40	3.4.2	41	6	42	5	43	4	44	7	45	2.1
46	2.3.1	47	7	48	3	49	8	50	2	51	6	52	5	53	5.6.7.4	54	7	55	1
56	3.1	57	7	58	4.3	59	9.8	60	3.2	61	6	62	5	63	4	64	7	65	2.1
66	2.1	67	7	68	3	69	8	70	2	71	6	72	5	73	5.6.4	74	8.7	75	1.*
	7.8.9		**6**.7.8.9		**2**.3.4.5 6.7		**4**.5.6.7		**1**.2.3.4 5.6		**5**.6.7.8 9		**1**.2.3.4 5.6.7.8.9		**3**.4.5.6 7.8		**7**.8.9		**2**.3.4.5
76	8.7	77 90	6 6	78	2	79 86	4 4	80 87	1 1	81 88	7.5 5	82	1	83	6.3	84	7	85 89	2 2
91	1	92	7	93	3	94	8	95	2	96	6	97	5	98	4	99	7	100	1

* At this point a change in experimenters occurred. The settings were also changed as is indicated. Beginning with trial 91 the original series of settings was again used.

TABLE 4

Daily Series and Averages with Ratios of Correct to Incorrect First Choices

Problem 1a

Crow Number 3 | Crow Number 4

Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug. 8	5	1	4				Aug. 8	5	2	3			
“ “	5	2	3				“ “	5	4	1			
“ “	5	2	3	5	10	1: 2	“ “	5	4	1	10	5	1:.5
“ 9	5	3	2				“ 9	5	1	4			
“ “	5	2	3				“ “	5	3	2			
“ “	5	3	2				“ “	5	3	2			
“ “	5	3	2	11	9	1:.81	“ “	5	4	1	11	9	1:.82
“ 10	5	3	2				“ 10	5	2	3			
“ “	5	4	1				“ “	5	4	1			
“ “	5	3	2				“ “	5	4	1			
“ “	5	3	2	13	7	1:.53	“ “	5	4	1	14	6	1:.35
“ 11	5	4	1				“ 11	5	1	4			
“ “	5	4	1				“ “	5	4	1			
“ “	5	4	1				“ “	5	4	1			
“ “	5	4	1	16	4	1:.25	“ “	5	3	2	12	8	1:.66
“ 13	5	2	3				“ 13	5	4	1			
“ “	5	2	3				“ “	5	3	2			
“ “	5	5	0	9	6	1:.67	“ “	5	5	0	12	3	1:.25
“ 14	5	5	0				“ 14	5	5	0			
“ “	5	5	0	10	0	1: 0	“ “	5	5	0	10	0	1: 0

RESULTS, PROBLEM 2

Our second problem, which we arranged as a more difficult one than the first, proved for the crows much more difficult than we had expected. It may be described as *the problem of the second door at the left.* The series of ten settings for this problem is as follows:

Problem 2. Second door at the subject's left to be chosen

Settings	Doors open	No. of doors open	No. of right door
1	7.8.9	3	8
2	6.7.8.9	4	7
3	2.3.4.5.6.7	6	3
4	4.5.6.7	4	5
5	1.2.3.4.5.6	6	2
6	5.6.7.8.9	5	6
7	1.2.3.4.5.6.7.8.9	9	2
8	3.4.5.6.7.8	6	4
9	7.8.9	3	8
10	2.3.4.5	4	3

For these ten settings the total number of doors open is fifty, of which ten are "right doors." The chance of a correct first choice, without previous experience or the prejudical influence of training in problem 1, is one to four.

In this problem, the first fifty trials were given to the crows in groups of two and three each. The birds made so many mistakes at the beginning that they became discouraged, and after two or three trials, would refuse to work. Later, as they became accustomed to the situation and the experimenter increased the degree of hunger, they could be induced to react five times in succession. Consequently the number of trials per series was increased to five.

Now, as in the case of problem 1, the experimenter was forced, in order to avoid the possibility of utterly discouraging his subjects, to aid them after they had worked for several minutes without success in locating the right door. This was always done by slightly moving the exit door of the right compartment.

After sixty-one trials had been given, the period of punishment was increased from fifteen seconds to thirty seconds. The thirty-second interval was used up to the four hundred and sixtieth trial. It was then increased to sixty seconds, but as the crows refused to work, it was decreased after forty trials to fifteen seconds.

After the fiftieth trial, the series regularly consisted of five trials, and four series were, as a rule, given each day.

Table 5 presents for problem 2, as does table 1 for problem 1, a summary of the choices for each of five hundred trials given each crow. As in table 1, the settings are indicated at the top of the various columns, and under each setting appear the results of the various trials for that particular setting.

It appears from table 5 that number 3, in the case of setting No. 1, 7-9, failed with few exceptions in its first choices until the three hundred and forty-third trial, whereas thereafter it usually succeeded. On the contrary, in the case of setting No. 6, 5-9, we observe that the bird almost never succeeded in selecting the right compartment in the first trial. Moreover, there is absolutely no evidence of improvement.

TAB

RESULTS FOR CROW NU

	S. 1		S. 2		S. 3		S. 4		S. 5
T.	7.8.9	T.	6.7.8.9	T.	2.3.4.5.6.7	T.	4.5.6.7	T.	1.2.3.4.5.6
1	9.8	2	9.7	3	7.6.7.7 7.7.3a	4	6.7.5a	5	6.2a
11	8a	12	7a	13	3	14	5a	15	2
21	9.9.9.7 9.8	22	9.7a	23	2.6.7.3a	24	4.7.4.4 4.5a	25	1.2a
31	7.9.8	32	6.9.7	33	4.3a	34	4.4.7.5	35	4.4.3.2
41	8	42	6.6.7	43	3	44	4.4.5	45	3.2
51	7.9.8	52	6.6.6.6 7	53	2.3	54	4.4.4.5	55	3.2
61	7.8	62	6.7	63	3	64	4.4.4.4 4.7.5	65	2
71	7.9.7.7 7.8	72	6.8.6.6 6.7	73	2.2.2.7 2.3	74	4.4.6.7 7.7.5	75	2
		67	6.7						
81	7.8	82	8.9.7	83	4.5.6.7 7.7.7.3	84	4.4.7.7 7.4.4.5	85	1.1.2
91	7.8	92	6.7	93	5.6.7.7.7.6 4.5.7.2.3	94	4.5	95	2
103	7.8	104	6.7	105	4.5.6.7.3	106	6.7.7.7.6 4.7.4.5	107	3.4.5.1.1.1 3.1.1.5.3.2
113	8	114	6.7	115	7.7.7.3	116	7.5	117	2
123	7.8	124	6.7	125	3	126	4.5	127	2
133	7.8	134	6.7	135	3	136	5	137	3.4.5.1.2
143	7.8	144	6.7	145	4.5.6.7.5 6.2.2.3	146	4.5	147	1.2
153	7.8	154	6.7	155	2.3	156	4.5	157	2
163	7.8	164	6.7	165	2.3	166	4.5	167	1.2
173	7.8	174	6.7	175	2.3	176	4.5	177	1.2
183	7.8	184	6.7	185	2.3	186	4.5	187	1.2
193	7.8	194	6.7	195	2.3	196	6.7.4.5	197	2
		204	6.7	205	3	206	4.5	207	3.4.5.6 1.2
213	7.8	214	6.6.7	215	2.3	216	4.5	217	2
223	8	224	6.7	225	4.5.2.3	226	4.5	227	3.4.5.1.2
233	7.8	234	6.7	235	3	236	4.5	237	2
243	8	244	7	245	2.3	246	7.5	247	1.2
253	7.7.7.7.7 7.7.7.7.7 7.7.7.8a	254	6.6.7	255	3	256	4.4.4.4 4.7.5	257	3.4.2
263	8	264	8.7	265	3	266	4.6.7.5	267	2
273	7.7.8	274	9.6.8.7	275	4.2.2.6 5.3	276	4.4.4.7.5	277	3.2
283	7.7.8	284	8.7	285	3	286	5	287	3.2

LE 5
MBER 3 IN PROBLEM 2

T.	S. 6 5.6.7.8.9	T.	S. 7 1.2.3.4.5 6.7.8.9	T.	S. 8 3.4.5.6.7.8	T.	S. 9 7.8.9	T.	S. 10 2.3.4.5
6	8.9.9.6a	7	2a	8	3.4a	9	9.8a	10	3a
16	6a	17	2a	18	4a	19	8	20	3a
26	6a	27	2a	28	4a	29	8	30	3
36	5.8.7.5 5.5.6a	37	3.2	38	3.3.3.3 3.3.3.4	39	7.8a	40	3
46	5.5.6a	47	2	48	4	49	7.7.7.7.7 7.7.9.7.8a	50	4.2.3
56	5.6	57	2	58	3.3.3.3.4	59	7.7.7.9.7 7.9.7.8	60	2.3
66	8.9.7.6			68	3.3.6.8 5.6.4	69	8	70	3
76	8.5.6	77	2	78	3.4	79	7.8	80	3
86	6	87	1.2	88	4	89	7.8	90	2.3
96	6	97	2						
98	5.6	99	2	100	3.3.6.4	101	7.8	102	3
108	7.8.9.5 8.6	109	3.4.5.1 2	110	3.3.3.3.4	111	7.8	112	2.2.3
118	5.5.6	119	3.4.2	120	3.4	121	7.8	122	2.3
128	7.8.9.6	129	3.4.5.2	130	3.4	131	7.8	132	2.4.5.2.3
138	7.8.5.8 9.6	139	1.2	140	3.4	141	8	142	2.3
148	6	149	3.4.5.6.7 8.9.5.5 6.7.8.2	150	3.4	151	7.7.7.8	152	2.3
158	8.9.5.5.6	159	2	160	3.4	161	7.8	162	2.3
168	5.6	169	2	170	3.4	171	7.8	172	3
178	5.6	179	1.2	180	3.4	181	7.8	182	2.3
188	5.6	189	2	190	4	191	7.8	192	3
198	5.6	199	2	200	4	201	7.8	202	4.5.5.5 2.3
203	5.6								
208	5.6a	209	1.2	210	8.8.3.3 3.4	211	8a	212	2.3
218	7.8.8.6a	219	2	220	4	221	8	222	4.5.4.2.3
228	8.9.6	229	2	230	4	231	7.8	232	4.5.2.3
238	8.9.7.6	239	3.4.5.6 7.2	240	3.3.3.3.4	241	8	242	3
248	5.6	249	2	250	3.3.3.6.5.4	251	7.7.8a	252	3
258	5.6	259	2	260	3.3.3.7.8 7.5.3.3.4	261	7.7.8	262	3
268	7.6	269	1.2	270	7.3.8.6.8 6.3.3.4	271	7.8	272	3
278	8.7.6	279	3.2	280	8.7.6.4	281	7.8	282	3
288	9.8.7.6	289	3.1.2	290	4	291	7.7.8	292	4.3

TABLE 5—

RESULTS FOR CROW NUMBER

	S. 1		S. 2		S. 3		S. 4		S. 5
T.	7.8.9	T.	6.7.8.9	T.	2.3.4.5.6.7	T.	4.5.6.7	T.	1.2.3.4.5.6
293	8	294	8.7	295	3	296	4.4.5	297	3.2
303	8	304	6.7	305	5.4.3	306	4.4.7.6.4 6.4.6.7.5	307	3.1.2
313	7.7.8	314	7	315	3	316	4.4.4.6.5	317	3.2
323	8	324	8.7	325	3	326	4.5	327	4.3.1.2
333	7.8	334	8.7	335	4.3	336	6.7.4.5	337	1.2
343	8	344	6.7	345	2.2.3	346	7.5	347	2
353	8	354	8.7	355	3				
361	8	362	8.7	363	2.2.3	364	6.5	365	3.2
371	8	372	7	373	4.3	374	7.6.5	375	3.2
381	7.8	382	7	383	4.2.2.3	384	4.4.4.5	385	2
391	8	392	8.7	393	3	394	6.5	395	2
401	8	402	8.7	403	3	404	6.5	405	2
411	8	412	6.7	413	2.2.3	414	5	415	3.2
421	7.8	422	6.7	423	3	424	4.4.4.5	425	2
431	8	432	8.7	433	3	434	4.5	435	1.2
441	8	442	8.7	443	4.3	444	6.5	445	2
451	8	452	7	453	2.4.3	454	4.5	455	2
461	8	462	6.7	463	2.4.3	464	7.6.5	465	2
471	8	472	8.7	473	2.4.3	474	7.6.4.5	475	5.4.5.6 5.5.4.2
481	7.8	482	8.7	483	3	484	7.4.5	485	2
491	8	492	9.8.7a	493	2.3	494	6.4.5	495	4.3.2

RESULTS FOR CROW NU

6	9.9.8a	7	9.7a	8	4.3a	9	5a	10	2a
16	8a	17	7a	18	3	19	5	20	5.6.6.6 2a
26	7.8	27	6.7	28	4.6.7.3	29	4.6.7.7 4.4.5a	30	4.5.3.2a
36	8	37	8.9.7	38	4.6.2.7 7.3a	39	7.5a	40	2a
46	7.9.8	47	7	48	4.6.5.7.2 7.7.2.2.3a	49	5	50	3.6.2
56	8	57	7	58	2.7.2.6.3a	59	4.4.4.7.5	60	2
66	9.8	67	7a	68	2.3	69	7.4.7.6.5a	70	3.2
76	7.7.8	77	6.8.6.8 9.8.7a	78	2.7.2.6 2.3	79	4.7.5a	80	2a
86	8	87	6.8.7	88	2.7.7.3	89	5	90	6.1.4.6 2

Continued

3 IN PROBLEM 2—*Continued*

T.	S. 6 5.6.7.8.9	T.	S. 7 1.2.3.4.5. 6.7.8.9	T.	S. 8 3.4.5.6.7.8.	T.	S. 9 7.8.9	T.	S. 10 2.3.4.5
298	8.7.6	299	2	300	4	301	7.7.8	302	4.3
308	5.6	309	8.7.5.3.2	310	7.6.4	311	8	312	5.3
318	7.6	319	2	320	8.7.6.5.4	321	7.8	322	4.3
328	7.6	329	4.3.1.2	3 0	3.4	331	8	332	5.5.3
338	8.6	339	1.2	340	3.3.3.4	341	7.7.7.8	342	4.3
348	7.6	349	4.3.4.3 1.1.2	350	3.3.3.4	351	7.8	352	5.4.3
356	7.8.7.6	357	2	358	8.7.5.4	359	8	360	3
366	7.6	367	4.3.2	368	5.4	369	8	370	5.4.3
376	8.7.6	377	3.2	378	6.5.4	379	8	380	2.3
386	8.7.6	387	2	388	7.6.5.4	389	9.8	390	5.4.3
396	8.7.6	397	2	398	5.4	399	7.7.8	400	2.2.2.3
406	7.6	407	2	408	8.7.6.5.4	409	7.7.8	410	3
416	7.6	417	4.3.2	418	3.3.4	419	8	420	4.3
426	5.5.6	427	3.2	428	3.4	429	8	430	3
436	8.6	437	3.2	438	3.4	439	7.8	440	2.4.4.3
446	6	447	2	448	6.5.4	449	8	450	2.2.4.3
456	8.7.6	457	1.2	458	6.7.5.4	459	7.8	460	2.2.3
466	7.6	467	3.2	468	3.5.4	469	7.8	470	2.3
476	8.7.9.8 7.8.8.7 8.8.6	477	2	478	4	479	8	480	5.4.3
486	8.7.8.5 7.7.9.8 8.6a	487	2	488	3.4a	489	7.8	490	2.2.3
496	9.8.8.8 5.6	497	2	498	3.3.4	499	9.9.8	500	3

MBER 4 IN PROBLEM 2

1	9.9.6a	2	2a	3	4a	4	7.8	5	5.5.2.2 5.5.3a
11	6a	12	3.2a	13	4a	14	8a	15	4.3a
21	6	22	5.6.6.8.2	23	4	24	7.8	25	4.5.5.2 2.3
31	5.9.5.9.8 5.9.5.6a	32	4.2	33	3.3.3.4a	34	8	35	5.5.2.5.2 5.5.2.3a
41	9.9.6a	42	1.6.2a	43	5.8.3.8 4a	44	7.9.7.8	45	4.3
51	5.9.8.6	52	3.2	53	4	54	8	55	4.5.3
61	9.5.7.9 7.9.8.7 9.5.8.6a	62	3.9.1.8 4.2	63	3.8.4	64	8	65	2.5.3a
71	9.8.6	72	9.1.6.3 7.2	73	3.8.8.4a	74	7.9.8	75	2.5.2.4.3
81	8.9.8.7 6	82	8.8.2	83	3.4	84	8	85	2.2.3a
91	8.8.6	92	8.4.7.1 8.9.1.4 8.4.2a	93	4	94	7.7.8	95	3

TABLE 5—
RESULTS FOR CROW NU

	S. 1		S. 2		S. 3		S. 4		S. 5
T.	7.8.9	T.	6.7.8.9	T.	2.3.4.5.6.7	T.	4.5.6.7	T.	1.2.3.4.5.6
96	9.8	97	8.8.8.6 8.7a						
98	7.9.7.7 7.7.7.8	99	8.9.9.7	100	7.4.2.6 2.4.4.3a	101	7.7.6.4 4.5	102	2
108	7.9.8	109	6.9.8.7	110	3	111	4.7.4.4 6.4.6.5	112	2
118	8	119	6.8.7	120	2.6.4.6 2.7.3	121	4.6.4.5a	122	1.1.4.2
128	7.8	129	7	130	3	131	4.6.6.4 6.7.4.5	132	1.3.3.1 5.2
138	7.8	139	7	140	2.4.2.3	141	4.6.5	142	1.3.2
148	7.8	149	8.6.8.6 6.8.6.6.7	150	5.2.7.2 5.2.4.2.3	151	4.5a	152	2
158	8	159	8.8 8.7	160	3	161	4.4.5	162	2
168	7.7.8	169	6.8.8.7	170	2.3	171	5	172	1.6.4.2
178	8	179	7	180	5.2.2.6 4.3	181	6.4.4.7 4.6.6.5	182	2
188	8	189	7	190	4.2.3	191	5	192	1.3.2
198	7.8	199	6.7	200	4.2.3	201	6.5	202	3.1.4.2
208	8	209	7	210	5.3	211	4.4.7.6 4.5a	212	2
218	8	219	8.6.6.9 7	220	4.3	221	7.5	222	2
228	7.8	229	6.7	230	2.4.3	231	4.4.4.7.4 4.6.4.4.5	232	1.3.2
238	7.8	239	7	240	3	241	7.7.4.5	242	2
248	8	249	7	250	2.3	251	4.5	252	1.2
258	7.8	259	6.7	260	2.4.5.6.3	261	5	262	2
268	8	269	6.7	270	2.3	271	4.5	272	1.2
278	7.8	279	6.7	280	2.3	281	4.5	282	1.3.1.2
288	7.9.7.8	289	6.7	290	2.3	291	5	292	1.2
298	7.8	299	6.7	300	2.4.2.3	301	4.5	302	1.3.5.3 4.1.2
308	7.8	309	7	310	2.3	311	6.7.5	312	1.2
318	8	319	8.8.7	320	6.7.4.2 2.3	321	4.5	322	1.3.4.2
328	8	329	7	330	2.4.2.3	331	5	332	6.3.4.1.2
338	8	339	8.8.6.7	340	2.2.3	341	5	342	3.2
348	8	349	7	350	2.3	351	4.5	352	1.2
356	8	357	6.7	358	7.5.2.3	359	7.4.5	360	6.3.4.1.2
366	8	367	8.7	368	2.4.6.7 4.5.3	369	5	370	3.4.5.2
376	7.8	377	5.7.8.6*	378	3	379	5	380	3.4.5.2
386	7.8	387	6.7	388	2.3	389	4.5	390	1.3.4.2

* Mistake, setting 5-9.

Continued

MBER 4 IN PROBLEM 2

	S. 6		S. 7		S. 8		S. 9		S. 10
T.	5.6.7.8.9	T.	1.2.3.4.5 6.7.8.9	T.	3.4.5.6.7.8	T.	7.8.9	T.	2.3.4.5
103	6	104	8.2	105	4	106	9.8	107	3
113	9.7.8.7 9.8.6	114	3.8.7.4 6.2	115	3.6.3.3 3.4	116	8	117	2.3
123	5.9.7.6	124	2	125	3.6.8.6.4	126	7.7.7.7.. 7.8	127	3
133	5.8.8.6	134	1.3.8.7 8.6.7.2	135	3.5.3.7 3.6.4	136	7.9.7.7 7.8	137	2.2.3a
143	9.5.8.6	144	1.8.5.7 1.4.5.3.2	145	3.4	146	8	147	2.3
153	8.7.8.7 6	154	2	155	3.4	156	7.8	157	3
163	8.9.9.6	164	2	165	3.3.4	166	8	167	2.2.5.2.3
173	8.8.6	174	2	175	4	176	8	177	3
183	8.6	184	2	185	3.3.3.5.4	186	7.7.7.8	187	2.2.3
193	6	194	3.6.7.8 5.4.6.3 4.3.2	195	4	196	7.9.7.8	197	4.3
203	7.9.7.7 8.6	204	2	205	4	206	7.8	207	3
213	6	214	3.4.9.5 3.2	215	3.3.4	216	.78.	217	2.3
223	9.7.5.8 7.8.5.7.6	224	3.1.2	225	5.3.3.3.4	226	9.7.8	227	2.3
233	5.6	234	3.2	235	5.3.7.3.4	236	8	237	2.3
243	8.8.6	244	2	245	3.4a	246	8	247	2.3
253	6	254	3.4.3.2	255	4	256	8	257	3
263	7.8.7.8.7 7.5.8.7.6	264	2	265	4	266	8	267	2.2.2.3
273	5.6	274	3.4.2	275	8.7.7.3 5.7.4	276	7.8	277	2.3
283	5.6	284	3.4.2	285	5.6.4	286	7.8	287	4.2.3
293	5.6	294	2	295	3.4	296	7.8	297	2.4.2.3
303	8.6	304	1.2	305	5.7.6.4	306	7.8	307	3
313	5.7.8.6	314	1.3.4.5 6.7.5.2	315	7.6.7.6 7.4	316	9.8	317	2.3
323	8.7.5.6	324	5.6.2	325	5.4	326	8	327	2.3
333	6	334	1.2	335	8.6.3.4	336	7.8	337	2.3a
343	7.8.6	344	1.2	345	3.4	346	7.8	347	5.2.3
353	8.9.5.6	354	7.8.6.4 5.4.6.2	355	4				
361	8.7.8.5.6	362	2	363	4	364	9.7.8	365	4.2.3
374	5.7.8.9 5.5.6	372	2	373	4	371	7.8	375	3
381	5.6	382	2	383	3.4	384	7.8	385	2.3
391	5.6	392	1.3.2	393	3.4	394	7.8	395	2.3

TABLE 5—
RESULTS FOR CROW NUMBER

	S. 1		S. 2		S. 3		S. 4		S. 5
T.	7.8.9	T.	6.7.8.9	T.	2.3.4.5.6.7	T.	4.5.6.7	T.	1.2.3.4.5.6
396	8	397	6.7	398	4.4.5.6.3	399	4.5	400	1.3.4.5.2
406	7.8	407	8.6.7	408	2.3	409	4.5	410	2
416	7.8	417	7	418	2.4.5.2.4 2.4.2.4.3	419	5	420	1.2
426	7.9.7.8	427	6.7	428	2.3	429	4.6.4.5	430	2
436	8	437	6.7	438	3	439	5	440	6.2
446	8	447	6.8.8.6 8.6.7	448	2.2.4.5 6.2.4.2.3	449	4.5	450	2
456	8	457	6.7	458	2.4.2.3	459	4.5	460	3.2
466	8	467	8.6.8.8 6.7	468	3	469	4.5	470	3.4.3.2
476	8	477	7	478	2.4.5.2.3	479	4.5	480	1.2
486	8	487	8.7	488	2.4.2.3	489	7.6.5a	490	1.2
496	7.8	497	7	498	2.3	499	4.6.7.5	500	2

Concluded
4 IN PROBLEM 2—*Concluded*

T.	S. 6 5 **6**.7.8.9	T.	S7 1.**2**.3.4.5 6.7.8.9	T.	S. 8 3.**4**.5.6.7.8	T.	S. 9 7.**8**.9	T.	S. 10 2.**3**.4.5
401	6	402	2	403	4	404	8	405	3
411	5.7.8.5.6	412	3.4.2	413	3.4	414	7.9.7.8	415	2.3
421	5.7.5.7 8.6	422	2	423	8.3.4	424	7.8	425	2.3
431	6	432	3.4.2	433	3.4	434	8	435	4.3
441	6	442	2	443	6.6.7.3.4	444	7.8	445	2.3
451	5.8.8.5 7.8.5.6a	452	2	453	3.4	454	8	455	2.3
461	5.6	462	3.1.2	463	3.4	464	7.7.8	465	2.3
471	5.7.5.6	472	2	473	3.4	474	8	475	2.4.2.3
481	8.6	482	2	483	3.4	484	7.8	485	3
491	5.6	492	2	493	3.4	494	7.8	495	2.3

TABLE 6

Daily Series and Averages, with Ratios of Correct to Incorrect First Choices

Problem 2

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
July 6	3	0	3				July 6	3	0	3			
“ “	3	0	3				“ “	3	0	3			
“ “	2	0	2	0	8	0:8.	“ “	2	0	2	0	8	0:8.
“ 7	2	0	2				“ 7	2	0	2			
“ “	2	0	2				“ “	2	0	2			
“ “	2	1	1				“ “	2	0	2			
“ “	2	1	1	2	6	1:3.	“ “	2	0	2	0	8	0:8.
“ 8	1	0	1				“ 8	1	0	1			
“ “	3	1	2	1	3	1:3.	“ “	3	2	1	2	2	1:1.
“ 10	3	0	3				“ 10	3	2	1			
“ “	2	0	2				“ “	2	0	2			
“ “	3	0	3				“ “	3	0	3			
“ “	2	1	1	1	9	1:9.	“ “	2	0	2	2	8	1:4.
“ 11	3	0	3				“ 11	3	0	3			
“ “	2	0	2				“ “	2	1	1			
“ “	3	0	3				“ “	3	1	2			
“ “	2	1	1	1	9	1:9.	“ “	2	0	2	2	8	1:4.
“ 12	2	1	1				“ 12	2	0	2			
“ “	3	1	2				“ “	3	0	3			
“ “	3	2	1				“ “	3	1	2			
“ “	2	0	2	4	6	1:1.50	“ “	2	1	1	2	8	1:4.
“ 13	5	0	5				“ 13	5	2	3			
“ “	5	1	4				“ “	5	3	2			
“ “	5	2	3	3	12	1:4.	“ “	5	1	4	6	9	1:1,5
“ 14	5	1	4				“ 14	5	0	5			
“ “	3	0	3				“ “	3	0	3			
“ “	2	1	1				“ “	2	0	2			
“ “	5	2	3	4	11	1:2.75	“ “	5	0	5	0	15	0:15
“ 15	5	0	5				“ 15	5	1	4			
“ “	5	2	3				“ “	5	1	4			
“ “	5	1	4				“ “	5	2	3			
“ “	2	2	0	5	12	1:2.40	“ “	2	0	2	4	13	1:3.25
“ 16	5	2	3				“ 16	5	1	4			
“ “	5	0	5				“ “	5	3	2			
“ “	5	0	5				“ “	5	2	3			
“ “	5	2	3	4	16	1:4.	“ “	5	1	4	7	13	1:1.85
“ 17	5	0	5				“ 17	5	1	4			
“ “	5	2	3				“ “	5	2	3			
“ “	5	0	5				“ “	5	2	3			
“ “	5	2	3	4	16	1:4.	“ “	5	0	5	5	15	1:3.
“ 18	5	1	4				“ 18	5	1	4			
“ “	5	0	5				“ “	5	1	4			
“ “	5	1	4				“ “	5	1	4			
“ “	5	1	4	3	17	1:5.66	“ “	5	2	3	5	15	1:3.
“ 19	5	1	4				“ 19	5	3	2			
“ “	5	0	5				“ “	5	2	3			
“ “	5	2	3				“ “	5	1	4			
“ “	5	0	5	3	17	1:5.66	“ “	5	4	1	10	10	1:1.
“ 20	5	0	5				“ 20	5	3	2			

TABLE 6—*Continued*

Daily Series and Averages, with Ratios of Correct to Incorrect First Choices

Problem 2—*Continued*

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
July 20	5	0	5				July 20	5	1	4			
“ “	5	3	2				“ “	5	3	2			
“ “	5	1	4	4	16	1:4.	“ “	5	2	3	9	11	1:1.22
“ 21	5	2	3				“ 21	5	0	5			
“ “	5	1	4				“ “	5	3	2			
“ “	5	0	5				“ “	5	3	2			
“ “	5	1	4	4	16	1:4.	“ “	5	1	4	7	13	1:1.85
“ 22	5	3	2				“ 22	5	2	3			
“ “	5	1	4				“ “	5	0	5			
“ “	5	2	3				“ “	5	0	5			
“ “	5	2	3	8	12	1:1.50	“ “	5	1	4	3	17	1:5.66
“ 23	5	2	3	2	3	1:1.05	“ 23	5	3	2	3	2	1: .66
“ 24	5	2	3				“ 24	5	2	3			
“ “	5	2	3				“ “	5	2	3			
“ “	5	1	4				“ “	5	4	1			
“ “	5	2	3	7	13	1:1.85	“ “	5	2	3	10	10	1:1.
“ 25	5	3	2				“ 25	5	3	2			
“ “	5	1	4				“ “	5	1	4			
“ “	5	0	5				“ “	5	0	5			
“ “	5	1	4	5	15	1:3.	“ “	5	0	5	4	16	1:4.
“ 26	5	2	3				“ 26	5	0	5			
“ “	5	1	4				“ “	5	1	4			
“ “	5	2	3				“ “	5	1	4			
“ “	5	2	3	7	13	1:1.85	“ “	5	0	5	2	18	1:9.
“ 27	5	1	4				“ 27	5	1	4			
“ “	5	1	4				“ “	5	1	4			
“ “	5	1	4				“ “	5	0	5			
“ “	5	1	4	4	16	1:4.	“ “	5	1	4	3	17	1:5.66
“ 28	5	2	3				“ 28	5	1	4			
“ “	5	1	4				“ “	5	3	2			
“ “	5	0	5	3	12	1:4.	“ “	5	1	4	5	10	1:2.
“ 30	5	0	5				“ 30	5	1	4			
“ “	5	2	3				“ “	5	0	5			
“ “	5	0	5				“ “	5	2	3			
“ “	3	1	2	3	15	1:5.	“ “	3	1	2	4	16	1:4.
“ 31	5	3	2				“ 31	5	1	4			
“ “	5	1	4				“ “	5	2	3			
“ “	5	1	4	5	10	1:2.	“ “	5	2	3	5	10	1:2.
Aug. 1	5	2	3				Aug. 1	5	3	2			
“ “	5	1	4				“ “	5	2	3			
“ “	5	2	3				“ “	5	1	4			
“ “	5	1	4	6	14	1:2.33	“ “	5	0	5	6	14	1:2.33
“ 2	5	3	2				“ 2	5	0	5			
“ “	5	1	4				“ “	5	1	4			
“ “	5	3	2	7	8	1:1.14	“ “	5	5	0	6	9	1:1.5
“ 3	5	2	3				“ 3	5	1	4			
“ “	5	2	3				“ “	5	0	5			
“ “	5	1	4	5	10	1:2.	“ “	5	2	3	3	12	1:4.
“ 4	5	2	3				“ 4	5	1	4			

TABLE 6—*Continued*

Daily Series and Averages, with Ratios of Correct to Incorrect First Choices

Problem 2

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug. 4	5	2	3				Aug. 4	5	1	4			
“ “	5	2	3				“ “	5	2	3			
“ “	5	0	5	6	14	1:2.33	“ “	5	3	2	7	13	1:1.85
“ 5	5	2	3				“ 5	5	2	3			
“ “	5	3	2				“ “	5	2	3			
“ “	5	3	2	8	7	1: .87	“ “	5	2	3	6	9	1:1.5
“ 6	5	0	5				“ 6	5	1	4			
“ “	5	2	3				“ “	5	0	5			
“ “	5	0	5				“ “	5	2	3			
“ “	5	1	4	3	17	1:5.66	“ “	5	1	4	4	16	1:4.
“ 7	5	2	3				“ 7	5	2	3			
“ “	5	2	3				“ “	5	2	3			
“ “	5	1	4				“ “	5	1	4			
“ “	5	1	4	6	14	1:2.33	“ “	5	1	4	6	14	1:2.33
“ 8	5	2	3	2	3	1:1.5	“ 8	5	2	3	2	3	1:1.5

The summary of choices given in table 5 is chiefly valuable as a means of detecting reactive tendencies. But it also indicates that neither crow succeeded in solving the problem. We had supposed, from our previous experience, that within two or three weeks the crows would be choosing the second compartment at the left with ease, but as a matter of fact, with the appearance and disappearance of the more or less unsatisfactory reactive tendencies apparent in table 5, they continued their work over a period of several weeks without mastering the situation. It seemed utterly useless to continue the experiment with this problem beyond the five hundredth trial. Had there been any consistent improvement, even although extremely slow, we should have felt justified in continuing the training.

The presentation of results in table 6 is of interest primarily because the reader can from it see the fluctuation in the measure of success during the long continued period of training. We have presented in this table for each bird the number of correct and incorrect first choices by series of trials under each date. Following the results appear the ratios of successes to failures for each day.

In table 7 the ratios of correct to incorrect first choices are given for the trials by groups of twenty-five, in order that the influence of "good" and "bad" days may be fairly distributed.

TABLE 7

RATIOS OF CORRECT TO INCORRECT FIRST CHOICES IN PROBLEM 2 BY GROUPS OF TWENTY-FIVE

Trials	Crow No. 3	Crow No. 4
1- 25	1: 7.30	1: 5.25
26- 50	1: 3.16	1: 5.25
51- 75	1: 4.00	1: 3.16
76-102	1: 2.00	1: 4.40
103-127	1: 5.25	1: 1.77
128-152	1: 5.25	1: 1.40
153-177	1: 5.25	1: 1.08
178-202	1: 3.16	1: 1.77
203-227	1: 3.16	1: 1.77
228-252	1: 1.50	1: 2.12
253-277	1: 2.57	1: 1.50
278-302	1: 1.12	1:11.50
303-327	1: 3.16	1: 5.25
328-352	1: 7.33	1: 2.57
353-375	1: 1.87	1: 1.55
376-400	1: 2.12	1: 5.25
401-425	1: 1.15	1: 1.77
426-450	1: 1.77	1: 1.50
451-475	1: 3.16	1: 3.16
476-500	1: 2.12	1: 2.12

The probability of a correct choice in this experiment, supposing that chance alone is involved[5], is one to four. At the beginning of the experiment, it is noted (table 7) that the ratio for number 3 was 1 to 7.30; that for number 4, 1 to 5.25. For neither bird does the ratio fall as low as 1 to 1 at any time during the training. The nearest approach to this measure of success was made by crow number 4 in the trials 153 to 177, for which the ratio was 1 to 1.08.

Further, it is to be noted that neither crow shows a steady increase in the number of correct choices. There is, instead, for each, an increase up to a certain point, then a sudden decrease, followed by a more or less rapid increase. Number 3 exhibits three well marked improvement waves. Beginning with the ratio 1 to 7.3, there is fairly constant improvement until the ratio stands 1 to 2. Then a backsliding occurs which, for the next twenty-five trials results in a ratio of 1 to 5.25. Slowly the bird improves again, achieving, after about three hundred trials, a ratio of 1 to 1.12. But this, after fifty additional trials, is replaced by a ratio of 1 to 7.33. Immediately thereafter, rapid improvement sets in, and shortly a ratio of 1 to 1.15 results.

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[5] Of course the previous work on Problem 1 influenced the birds very markedly in their early trials.

The same in general holds for number 4. At the end of fifty trials, its ratio is 1 to 5.25. After one hundred and seventy-seven trials, it is 1 to 1.08. Then the number of correct first choices slowly decreases until finally, at about the three hundredth trial, the ratio is 1 to 11.50. There follows, during the next seventy-five trials, improvement which results in the ratio of 1 to 1.55, which, in turn, is immediately followed by the ratio of 1 to 5.25.

These fluctuations in the ratio of right to wrong first choices are indicative of the appearance, "trying out," and disappearance of more or less satisfactory reactive tendencies. The fact that neither bird achieved a ratio of 1 to 0 indicates that no reactive tendency appeared which was wholly satisfactory, or in other words, led to the complete solution of the problem. These various reactive tendencies we should describe, in the case of a human subject, as the "trying out" of ideas, but it is unnecessary for us to have recourse to this mode of psychological description in the case of the crows. They may or may not have had ideas corresponding to those which would have existed in the ordinary human subject. In any event, their behavior is strikingly similar to that of the human subject of a low level of intelligence.

ANALYSIS OF THE REACTIONS OF CROW NUMBER 3, ♂

An analysis of the data of table 5 renders these fluctuations of the ratio of correct to incorrect first choices at once intelligible and deeply significant. We shall attempt an analytical examination of the results for each subject in order to bring the several reactive types and tendencies into prominence.

For crow number 3 the first thirty or forty trials in problem 2 in large measure destroyed the subject's well formed habit of choosing first, as a result of previous training in problem 1, the first compartment at the right. The bird then began to choose very frequently the first compartment at the left and to distribute the remainder of its choices among the other compartments, until the right one happened to be chosen. From the beginning of work on this problem, the persistency of number 3, as also of number 4, in reentering the same compartment was surprising. Examples of this are trials three, twenty-one, twenty-four, thirty-eight, forty-nine, fifty-nine, sixty-four, and so on, for number 3.

By the time number 3 had been given one hundred trials, a habit of always going to the first compartment at the left, and after receiving the punishment of confinement in the compartment, entering the one next in order, had become fairly well fixed. For some of the settings, this habit developed earlier than for others. For instance, in settings Nos. 1 and 9, 7-9, this particular reactive tendency appeared first in the thirty-ninth trial, again in the sixty-first, and in the seventy-ninth, when it seems to have been accepted as the most satisfactory method of reacting, and appears as a habit for almost two hundred trials.

With setting No. 2, 6-9, this same reactive tendency appears definitely at about the ninety-second trial; for setting No. 3, 2-7, at about the one hundred and fifty-fifth; for No. 4, 4-7, at the one hundred and twenty-sixth; for No. 5, 1-6, at the one hundred and twenty-sixth; for No. 6, 5-9, at the one hundred and sixty-eighth; for No. 8, 3-8, at the one hundred and twentieth; for No. 10, 2-5, at the one hundred and forty-second; but for No. 7, 1-9, neither this tendency nor any other became well established during the five hundred trials.

In the fifty trials, one hundred and fifty-one to two hundred inclusive, the habit of entering the first compartment at the left, and next the adjacent one, which of course was also the correct one, appeared thirty-eight times. In ten of these fifty trials, the right compartment was entered immediately, and in the remaining two trials, the compartment first entered was the second from the right instead of the second from the left end of the group.

From trials two hundred and forty to two hundred and fifty, a new reactive tendency began to appear. This shortly replaced the one just described. Previously, number 3, when it came out of the first compartment at the left, turned sharply to its left and entered the second compartment, the right one, but now instead of turning to its left, it began to turn to its right, with the result that it faced the compartment which it had just left. Formerly, it had always met with reward when, after coming out of the first compartment and turning sharply it entered the one directly in front of it, but now it met, instead, with punishment. Nevertheless, it persisted in reentering the wrong compartment, and in trial two hundred and fifty-three it was punished thirteen times for entering compartment 7. It was then aided in finding the right compartment. In this instance, even after the door of the wrong compartment which had been so often reentered had been closed and the door of the right compartment left open beside it, the bird stood for some seconds before the wrong door, cawing and apparently eager to enter.

Naturally the tendency to turn to its right instead of to its left greatly diminished the number of correct first choices by number 3, and completely obliterated the old reactive tendency. Shortly, the number of reentrances diminished to two or three, and the bird began to enter the second compartment from the left, even although it were not facing it after it turned about. This peculiar behavior continued for only a short time, and was followed by a tendency to enter first a compartment near the right end of the series. On escaping from this, it would turn to its right and enter the compartment directly in front of it. Repetition of this performance of course soon brought the bird to the right compartment. In trial after trial, number 3 would enter the first compartment at the right and then work back, compartment by compartment, until it reached the second from the left. Examples of this behavior are trials two hundred and eighty, two hundred and eighty-eight, three hundred and nine, three hundred and twenty, and so on.

After the thorough testing of the reactive tendency just described, no additional habit became well established, but the crow shifted from one method to another. The one most often used was that of entering the third from the left, and on leaving this, turning to the right and entering the compartment before it, which was of course the right one. This method is exhibited for setting No. 2, 6-9, after the two hundred and eighty-fourth trial; for setting No. 5, 1-6, after trial two hundred and seventy-seven; for setting No. 6, 5-9, after trial three hundred and eighteen; and for setting No. 10, 2-5, after trial two hundred and ninety-two. For the other settings, it appeared less frequently.

To complete our comments on the behavior of number 3, we may say that beginning with trial four hundred and sixty-one, the period of punishment was increased from thirty seconds to sixty seconds, since it seemed possible that the crow might improve under this condition. But the punishment was over-severe, and after only a few trials, number 3, as also number 4, began to work very badly indeed, It would move about constantly and excitedly while confined in a compartment, and when the door was opened would rush out and immediately enter another compartment without pause. This random and excited choosing naturally yielded few successes, and by the time forty trials had been given, number 3 was very hesitant about entering any of the compartments and had returned to an earlier habit of wandering about the reaction chamber. When the time of punishment was reduced to fifteen seconds, he very quickly resumed his former method of reacting, and worked quite as assiduously as ever, and with as small a measure of success.

ANALYSIS OF THE REACTIONS OF CROW NUMBER 4,♀

The behavior of number 4 in problem 2 differs in some respects from that of number 3 and is worthy of brief description. The first fifty trials served to break up the habit of choosing the first compartment at the right. Thereupon, her attention shifted to the opposite end of the series. But this was not so definite as in the case of number 3. Number 4 often went to the first compartment at the left and then to the one next to it, thus requiring but two choices in order to get the right compartment. This tendency became fixed only after two hundred and fifty trials, and even then it was not so definite as for number 3.

It is indicative of the temperamental differences in the two subjects that number 4 should have required assistance in almost twice as many of the first two hundred trials as did number 3. Significant also is the fact that until very late in her training she was not nearly as systematic in her choices as was he. She tended rather more frequently to the compartments near the middle instead of those at the ends, and chose in no definite or predictable way. This naturally resulted in a much larger number of choices before the right compartment was reached, than in the case of number 3. Discouragement was proportional to the number of mistakes. But at the same time, number 4, just because of the lack of a definite inadequate reactive tendency, happened upon the right compartment more frequently than did number 3. For her, therefore, the ratio of correct to incorrect choices is more favorable than for him. This is true up to about the three hundredth trial, when it appears that number 4 for some reason became more systematic in her work, going most frequently to the first compartment at the left and then to the second. This habit, which had appeared also in the behavior of number 3, had by this time been replaced, and as a result he was more often choosing correctly than she. Number 4 continued to exhibit this reactive tendency rather insistently throughout the remaining trials.

We must conclude from this analysis of the data of table 5 that both crows, in the five hundred trials with problem 2 which were given them, tried and found inadequate all of the reactive tendencies which were immediately available. Toward the end of the experiment, it was evident, especially in the case of number 3, that the bird's only resource was to return to some one of the methods previously employed. Strange as it may seem to the human subject, and especially to those human beings who have a high estimate of the intelligence and originality of the crow, these individuals proved entirely incapable of learning to enter directly the second compartment from the left in a series of compartments.

Doubtless, many readers will object that longer training would almost certainly have enabled our subjects to solve this problem. We cannot deny this possibility, but we must insist that all of the indications of our results are against it, for ordinarily the adequate solution of such a problem as this by an animal of intelligence far lower than the human is achieved by slow improvement. We are of the opinion that the crow is incapable of perceiving and properly reacting to the relation of second from the left, and we do not hesitate to admit that we were very much surprised by this outcome of our experiments, as we had fully expected our subjects, and especially crow number 3, to deal successfully with much more difficult problems than this one.

This opinion rests not solely upon the fact that no steady and consistent improvement occurred as the result of five hundred trials distributed over a period of thirty-two days, but also upon the observation that in the case of the setting 7-9, which appeared twice in the series, as Nos. 1 and 9, and was therefore presented to each crow one hundred times instead of fifty times, the successes were surprisingly few. In the first presentation of this setting, number 1 in the series, for both of the crows there is marked increase in the number of correct first choices between the beginning and the end of the training. But for the second presentation, number 9 in the series, this is not the case. Crow number 3, in the first ten presentations of this setting, as number 9 in the series, chose correctly only twice, and in the last ten, only four times, while crow number 4 chose correctly in the first ten, four times, and in the last ten, four times. Even without experience they should have chosen correctly twice in ten trials.

This is an easy setting and it is surprising indeed that the crows should not have succeeded in reacting correctly in the latter part of the training. Doubtless their failure is due to the confusing effect of the diverse settings.

We feel that our analysis and discussion of results is inadequate, but the report is already overlong because of the necessarily lengthy description of apparatus and experimental procedure, and we may add only a brief summary.

SUMMARY

1. Two crows, No. 3, a male, and No. 4, a female, about three months old, were presented with two of the simplest types of standard problem in the Yerkes multiple choice apparatus. These problems were: (1) selection of the compartment first at the right in a series; (2) selection of the compartment second at the left; and (3) the other form of problem 1, the selection, namely, of the compartment first at the left.

2. Of these three problems both birds succeeded in solving perfectly, in from fifty to one hundred trials, the first and the last. The second problem they failed to solve in five hundred trials.

3. Various types of reaction and reactive tendencies appeared during the work on problem 2. Examples of these are: (1) to go to the first compartment at the right because of training in problem 1; (2) to go to the first at the left, and then to the

next in order ;(3) to reenter the compartment first chosen and then to choose the second from the left of the series; (4) to enter a compartment at or near the right end of the series and on emerging to turn to the right and enter the one directly in front, and so on until the right compartment is entered.

4. Since no one of these types of reaction is satisfactory, the birds shifted from one to another, trying them for varying periods.

5. The male was more tame, bold, and aggressive than the female. Consequently, he made the better showing in the experiments.

The multiple choice method, with four standardized problems, has been employed with pigs, rats, and ring-doves, as well as with crows; and, among human subjects, with normal and defective children and adults and with dementia praecox patients. The results will appear in a series of papers of which this is the first.

COLOR BLINDNESS OF CATS

J. C. DeVOSS AND ROSE GANSON

From the Psychological Laboratory of the University of Colorado

FOUR FIGURES

INTRODUCTION

The term "color-blindness" is used in the title of this paper, not because color-vision should be denied of an animal as a result of a single investigation, no matter how carefully it may have been conducted, but because the results of our experiments certainly make the term "color-blindness" a less presumptuous one than "color-vision" when applied to these animals.

The experiments were carried on in diffuse daylight and during the same hours each day, so that the results apply to the light-adapted eye of the cat. The fact that every discrimination and confusion made by one cat were also made by another, "the follower," would indicate that the conditions were not variable enough to cause a difference between the responses of the two animals. This agreement would indicate further that our results represent general characters of feline vision rather than individual peculiarities.

The investigation was begun in February 1911 and continued until June 1913. Thus it required twenty-eight months to test the animals with the large number of colors and grays which we employed.

We are indebted to Professor Lawrence W. Cole, under whose supervision the work was done, for invaluable advice and encouragement as well as for the suggestion of the problem. We also wish to acknowledge our obligation to Miss Mary E. Lakenan, who did some preliminary work toward devising a method suited to the animals to be tested.

Among the mammals, the vision of mice, rats, rabbits, squirrels, dogs, cats, raccoons, and monkeys has been investigated. Only one cat was included but Colvin and Burford[1] found it equal in color-vision to the dogs and the squirrel which they tested. They concluded that "The tests seem to show a surprising fineness of color-discrimination among the animals tested".

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[1] Colvin, S. S. and Burford, C. C. The color-perception of three dogs, a cat, and a squirrel. *Psych. Rev. Monog. Supp.*, vol. 11, Nov., 1909, pp. 1-49.

The chief results of recent experiments on mammalian color-vision have been the refinements in methods achieved through comparison and keen criticism. We cannot here deal with the technique of experiments other than our own, but the following generalizations will indicate the direction which the improvement in methods has taken.

1. So great a difference exists between the results obtained from the light-adapted and the dark-adapted eye that those secured under the one condition give us almost no clue to those which may result from the other.

2. All secondary criteria must be rigidly excluded. Differences of form, depth, size, texture, and especially differences of brightness between the lights or test papers used are especially to be guarded against.[2]

3. Much stress has been placed on the need of "natural conditions" in testing an animal's vision. It is reasonable to suppose that artificial conditions, like testing the animals in a dark room, or by artificial light, might lead to false views of his visual powers.

4. The stimulus color may be (a) reflected light from colored objects, usually papers, or (b) transmitted light which may be filtered through colored glasses or colored fluids, or isolated from certain spectra. The first method is used to test the light-adapted eye, or daylight vision, while the second is used chiefly to test the dark-adapted eye, or twilight vision.

The second class of methods strives to secure different hues of pure or homogeneous light. Both plans seek to find pairs of colors, or colors and grays which will be confused by the animal, assuming that the intensities of the colors for the animal and for human vision may not be the same.

Many criticisms of the use of colored papers for testing vision have been made. Their surfaces are said to differ greatly owing to accidents of manufacture. It is said to be difficult to bend them around glasses. They do not reflect homogeneous light but overlapping bands, etc. etc. None of these criticisms, however, applies very clearly to pairs of colors which are *confused* by the animal. Rather they are meant to point out all of the "secondary criteria" by which the animal *may discriminate* the colored papers. When an animal has confused a color and a gray for some six hundred trials, though quite accustomed to making discriminations, it seems fair evidence that he is unable to discriminate them by secondary criteria, nor even by the primary one, and that they appear the same to him.

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[2] See Yerkes, The Dancing Mouse, pp. 91-92, and 151.

After a confusion had been made between two colors (or a color and a gray) it seemed desirable to define it accurately for the sake of reproducing it at will. This we have done by stating its "flicker equivalent," a term used by Polimanti[3] to avoid any theoretical implication. A comparison of the equivalents we obtained with those of Cole and Long[4] shows that the flicker test reveals any fading of a colored paper and that the Bradley papers, except the violet hues, remain constant for a long time, if kept in darkness when not in use. In determining the flicker equivalents of the colored papers the decision was invariably based on inspection by two observers, and each color was tested with the gray just darker and just lighter than that with which it gave the minimum of flicker, a process which Ives[5] aptly calls "stepping off in various directions." Hence the decision in each case was the result of the comparison of at least three amounts of flicker. The method of determination was that described by Cole and Long (pp. 660-664). When a color gives a slight amount of flicker with each of two consecutive grays it is designated as between the two by giving the numbers of both. Thus Gray 1-2 means a gray which gives a slight amount of flicker when rotated with Hering Gray 1 or Hering Gray 2, a large amount when rotated with Gray 3, but which gives no flicker with those papers which in turn give very slight amounts with Grays both 1 and 2. These intermediate flicker equivalents occur because there are but fifty gray papers with which to test ninety or more colored papers. Cole and Long met this condition by establishing three amounts of flicker between each pair of grays. (p. 662). As we cannot adapt that device to the tables to be given in this paper, we shall designate a few of the colors by the numbers of both of the adjacent grays.

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[3] Polimanti, O. Ueber die sogennante Flimmer-Photometrie. *Zeitschrift fuer Psychologie*, vol. 19, 1899, S. 263ff.

[4] Cole, L. W. and Long, F. M. Visual discrimination of Raccoons. *Jour. of Comp. Neur. and Psych.*, vol. 19, 1909, p. 657ff.

[5] Ives, H. E. Photometry of lights of different colors. *Phil. Mag.*, 1912, p. 858.

In our search for grays which would be confused with the stimulus color we were finally driven to make tests with a number of gray cambrics. These also can be accurately defined by their flicker equivalents, and by no other method which we can discover. Of course the cambrics were used in both double and triple thicknesses, to prevent their transmitting light, both in experiments with the cats and in ascertaining their flicker values.

Quite aside from disputes as to the cause of the phenomena of flicker or what it measures, it makes colored and gray surfaces comparable, accurately definable and hence reproducible. It permits any series of colors or grays to be interpolated with any other since rotation obliterates differences of texture. It is accurate because very slight amounts of flicker are readily perceptible. Consequently we regard it as necessary to define our colors by their flicker equivalents, though our work is exploratory and qualitative.

While we have used flicker values merely for the sake of definition, Ives (p. 852) says of flicker photometry:

1. "It surpasses all other photometric methods in sensibility and reproducibility in the presence of color difference."

2. "It agrees at high illuminations with the equality of brightness method, when the latter is freed from the psychological uncertainties inherent in its use."

3. It measures at high illuminations what may fairly be termed the true brightness."

4. Brightnesses measuring equal to the same measure equal to each other and the sum of the measurements of the parts is equal to the measurement of the whole."

While these conclusions are drawn for the conditions of Ives's experiments they at least do not diminish the probability that our own flicker equivalents are at least rough approximations to measurements of brightness, though we have used them for the purpose of defining our papers.

In the following tables and description we shall use the initials F. E. for flicker equivalent and except where otherwise stated we shall not regard it as a measure of any property of the colored or gray papers but merely as the most accurate method of identifying any one of them.

We began this investigation with the intention of studying the vision of the cat under both daylight and twilight illumination, thus planning to use both reflected and transmitted light. Some work was done on the dark-adapted eye with filtered light, and we hope to carry the investigation further. At present we can report only the results obtained under light adaptation and by the use of colored papers. The enormous variation in the size of the cats' pupils suggested the possibility that this is a device which keeps the retina always in twilight. But as yellow, apparently, is the brightest of the colors for the cat as well as for the human eye, under light adaptation, the possibility of the animal's possessing twilight vision alone is very doubtful.

Three female and six male cats were tested. There is abundant evidence that these cats represented entirely non-related strains. Each cat was given from thirty to one hundred twenty trials a day according to his degree of hunger. One cat was given fifteen thousand trials and the trials of all the cats number over one hundred thousand.

By taking care not to over-feed the animals they were kept (with one exception) in excellent health throughout the progress of the experiments. The cats often purred during the experiments, which perhaps indicates that the conditions were not seriously "artificial".

The colors were presented in pairs and each cat was given preliminary training with easily discriminable pairs until his selection of the "food color" or stimulus color was rapid and accurate. Since most of the pairs were readily discriminated no cat reached a "confusion area" until he had been thoroughly trained in the experiment. With one animal we would begin at the red end of the Bradley series. With the other, which was to confirm or refute the results gained with the first, we began at the violet end, and so on. Since red is known to have a low stimulating power in the case of mice, rabbits, etc., we began our experiments with yellow and blue as stimulus colors.

From the beginning our object was to search systematically for confusions between a gray and a color, or between two colors, without any preconceived theory as to where, in the series, such a confusion might occur, or even whether it might occur at all. During hundreds of our first trials no confusions did occur, for we used Hering grays paired with Bradley colors. Discrimination was prompt and apparently this was due to the difference in texture between the two types of paper, for we later found numerous confusions. As shown by the cats' responses, pieces of ordinary cambric may be found which are so near the texture of the Bradley papers (when both are placed behind clear glass) that they are not discriminable by the animals. This is not true of Hering grays with Bradley papers. In such pairs we think that the Hering papers are not suitable for the study of any but the most defective eyes, because of their granular texture. This opinion is based on tests of the whole series of Hering grays with three cats. Miss Washburn[6] obtained admirable results with Hering grays and Bradley colors in investigating the vision of the rabbit. But as the rabbit has medullated nerve fibres passing in front of the retina[7] we should imagine that that defect would make the rabbit's eye very poor for the discrimination of textures.

In order to avoid awkward circumlocutions we have used such subjective terms as "confusion," "discrimination" (both "easy" and "difficult") etc. The reader will observe, however, that in every case these terms signify a definite objective condition, measured by the responses of the animals.

APPARATUS AND METHOD

The apparatus was that used by Cole and Long (p. 667). It was painted dead black and had black partitions between the glass holders. These partitions were necessary because in our experiments the colored papers were placed within the glasses. This permitted the possibility of a certain amount of reflection from a glass to the one beside it in case no black partitions were interposed. Since one cat seemed to make the choice of a glass by the position of the thumb-buttons, which fasten the levers, the buttons were concealed by shields of black cardboard cut for the purpose. The glasses mentioned were the feeding vessels and were ordinary jelly glasses, selected from a large assortment for clearness, freedom from flaws, and uniformity of size and shape. By means of the levers these glasses were clamped with their tops pressed closely against the under surface of the topboard, so that the animal must select the glass at which he pulled by its outside appearance only and without being able either to reach into it or look into it until he had pulled it down.

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[6] Washburn, M. F. and Abbot, Edwina. Experiments on the brightness value of the red for the light-adapted eye of the rabbit. *Jour. Animal Behavior*, vol. 2, 1912.

[7] Howell, W. H. Physiology, 1908, p. 319.

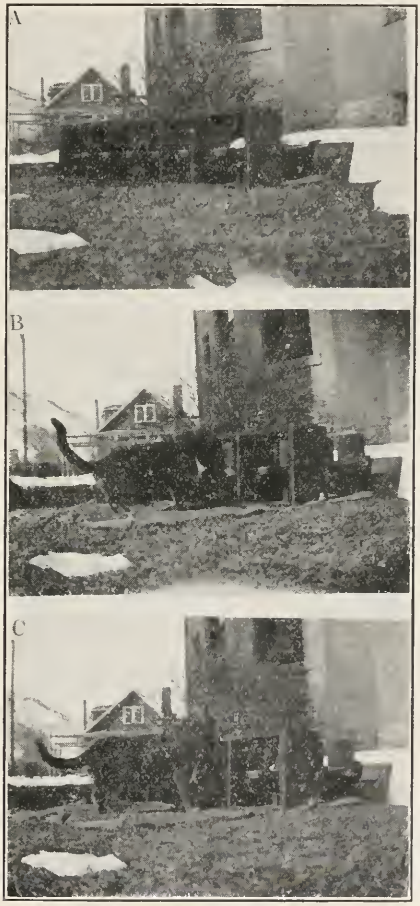

FIGURE 1. A. Apparatus for displaying the glasses which were tinted with colored papers. B. Cat depressing lever to release food-glass. C. Cat looking over the top of the apparatus in order to discriminate by the positions of the thumb-buttons.

By means of the thumb-buttons at the rear of the apparatus every glass except one was locked against the topboard. A pull at the glass which was on the free lever depressed its short arm and exposed the top of the glass so that the cat could reach into it or tip it over and secure the food which it contained. This glass will be referred to as the "food-glass" or the stimulus glass. The others will be called "confusion glasses," or confusion colors. The latter term will often include a gray where the context or the heading of the table has already specified a gray. From the front of the apparatus the colored papers, which lined the glasses made the only visible difference between them.

As already stated, the Bradley colored papers were used, and one blue cambric. After failing to find any confusions of the stimulus color with Hering grays we tried the cats with any gray we could obtain. The justification for this procedure is that we finally found a gray which two cats confused with each of the food-colors during at least six hundred trials. Only after this had been done was its flicker equivalent determined for the purpose of describing it.

At the beginning of the tests of each cat each day several glasses were cleaned and fitted with the necessary papers. The experimenter then took a position at one side and in front of the apparatus. Two glasses were displayed on adjacent levers and the cat was allowed to approach the apparatus from in front. If he went directly to the food-glass and drew it down, the choice was recorded as correct. But if he touched the confusion-glass ever so lightly, either with paws or head, the choice was called incorrect. After making his choice, the cat was allowed to discover the food-glass and secure food, but no record was made of this act. The cat was then placed where he could not see the levers while the glasses were being changed about. After this precaution, he was started again from his former position.

Thirty consecutive choices were considered a "series" and twenty-four or more right choices were required in a series before it was recorded as discrimination. If a cat consistently failed to discriminate two colors for twenty such series (six hundred trials), the result was recorded as complete confusion. In some instances an animal failed to make a discrimination for eight or more series, but showed by the low percentage of errors in each series that it might learn to discriminate and eventually did choose correctly twenty-four times in each of two consecutive series, thus making a discrimination. These were called "difficult discriminations." They doubtless indicate that the cats had to learn to discriminate what was for them close to a match. It was not difficult to identify a complete confusion, for the number of errors in each series almost invariably was twelve or more, while for six hundred tests the errors were not far from fifty per cent. On the other hand, the series in a difficult discrimination would contain a smaller proportion of errors, but it was here that greatest care was required.

Let us summarize the precautions taken to guard against discrimination by criteria other than the hue or intensity of the paper. The glasses were kept scrupulously clean so as to show no marks of any kind on their outer surface. They were all of clear glass without flaws. All of the papers were cut by a single pattern and placed in the glasses so that the overlapping edges were on the opposite side from the cat and hence invisible to him. Finally, circular disks of pasteboard were forced down within the cylinders of paper so that they held the paper closely pressed against the inner surface of the tumblers, and thus did away with wrinkles or apparent differences of depth. Several observers reported that the tumblers when thus prepared appeared to be made of colored glass. The apparatus prevented looking into the glasses before making a choice, and the experimenter put the glasses in position and made all changes while the cats were where they could not see the apparatus.

To prevent choice by position, the glasses were presented in random order, (one on the right, then on the left, then on the left again and so on), and they were changed in their positions along the apparatus. Whenever a cat formed the habit of choosing the glass on the left (or the right), the food-glass was placed no the other side until the habit was broken up.

To eliminate odor as a factor in discrimination, equal amounts of food were placed in the food-glass and the confusion glass; clean glasses and clean papers promptly replaced any that had become soiled, and the papers were exchanged within the two glasses so that the confusion-paper replaced the food-paper, and vice versa. Aside from the odor thus taken into account, the odor of the pigments of the two papers might presumably aid discrimination, hence within the paper in the food-glass, we placed a cylinder or lining of the confusion-paper and likewise within the paper in the confusion-glass was placed a cylinder of the food-paper. Moreover, the various papers were kept in one drawer and thus any odor peculiar to a certain pigment must have been pretty well diffused through them all.

There seems to be an almost irrefutable proof that our precautions were successful in at least a large number of cases. This proof rests in the fact that we obtained many complete confusions and, as already stated, each confusion was controlled by second cat or follower, which made it certain that the confusion was not due to accidental circumstances nor to some individual peculiarity of the first cat.

In working through the series of Bradley colors we came gradually to those among which confusion was difficult (required at least two hundred forty trials to attain twenty-four correct choices in a series of thirty trials). Then we came to colors which were confused with the food-color. We shall call these groups of colors, respectively, areas of "difficult-discrimination" and "confusion-areas," though in some cases these "areas" overlapped, i. e., one color was confused with the stimulus color, the next was discriminated from it with difficulty, the next was confused with it etc.

Tables I, II, and III show typical cases of discrimination, difficult discrimination, and confusion.

TABLE I (A)

Typical Discrimination by Series

Cat 2

Stimulus Color—Yellow

Yellow with	Errors	Correct Choices	No. of trials
B	0	30	30
BT1	0	30	30
BT2	5	25	30
GBS2	0	30	30
GBS1	0	30	30
GB	0	30	30
GBT1	0	30	30
GBT2	11	49	60
BGS2	0	30	30
BGS1	0	30	30

TABLE I (B)

Typical Discrimination by Series with Approach to a Confusion Area

Cat 4*

Stimulus Color—Yellow

Yellow with	Errors	Correct Choices	No. of Trials	F. E.	Verdict
YGS2	0	30	30	10	Discrimination
YGS1	0	30	30	7	"
YG	6	24	30	3	"
YGT1	5	25	30	3	"
YGT2	14	46	60	7	"
GYS2	1	29	30	8–9	"
GYS1	4	26	30	5	"
GY	38	82	120	3	"
GYT1	207	393	600	2	Confusion
GYT2	207	393	600	1–2	"

TABLE II

A Typical Case of "Difficult Discrimination"

Cat No. 3

Stimulus Color, Blue. Confusion Color, VBT1

Series	Errors	Correct Choices	Per Cent of Error
1	11	19	$36\frac{2}{3}$
2	10	20	$33\frac{1}{3}$
3	14	16	$46\frac{2}{3}$
4	14	16	$46\frac{2}{3}$
5	9	21	30
6	15	15	50
7	13	17	$43\frac{1}{3}$
8	8	22	$26\frac{2}{3}$
9	10	20	$33\frac{1}{3}$
10	5	25	$16\frac{2}{3}$
11	11	19	$33\frac{1}{3}$
12	8	22	$26\frac{2}{3}$
13	14	16	$46\frac{2}{3}$
14	9	21	30
15	10	20	$33\frac{1}{3}$
16	12	18	40
17	8	22	$26\frac{2}{3}$
18	5	25	$16\frac{2}{3}$
19	12	18	40
20	5	25	$16\frac{2}{3}$
21	7	23	$23\frac{1}{3}$
22	12	18	40
23	14	16	$46\frac{2}{3}$
24	3	27	10
25	4	26	$13\frac{1}{3}$
26	5	25	$16\frac{2}{3}$
Total........	248	532	Average......$31\frac{7}{26}\%$
Grand Total.	780		

---

*This cat had discriminated the yellow from the Bradley colored papers, beginning with the Red Violets through to the place where this table begins, i.e., he had discriminated it from forty Bradley colors.

TABLE III

A Typical Confusion

Cat No. 4

Stimulus Color, Yellow. Confusion Color, Gray 1-2*

Series	Errors	Correct Choices	Per Cent of Error
1	15	15	50
2	14	16	46⅔
3	18	12	60
4	13	17	43⅓
5	12	18	40
6	19	11	63⅓
7	15	15	50
8	15	15	50
9	14	16	46⅔
10	16	14	53⅓
11	15	15	50
12	17	13	56⅔
13	14	16	46⅔
14	13	17	43⅓
15	15	15	50
16	16	14	53⅓
17	17	13	56⅔
18	13	17	43⅓
19	14	16	46⅔
20	15	15	50
Total...20	300	300	Av. No. Errs..50%
Grand Total.................600			

CONFUSIONS OF COLORS WITH GRAYS

In a recent report on the vision of the rabbit Miss Washburn[8] says, "In order to eliminate the brightness error in experiments on color vision in animals, it is not sufficient to show that the animal tested can distinguish a color from the gray that a color blind human being would see in place of the colors, but the animal must be proved capable of discriminating this color from all grays." For a year before Miss Washburn published her report, we had been searching for a gray which the cats would confuse with orange-yellow, yellow, or blue. In their turns, cats numbered one, two, and three discriminated these colored papers from each of the fifty Hering grays.

Meanwhile some of our experiments had revealed the fact that the cat could not discriminate the yellow from some other colors of the same flicker equivalent. It was consequently suspected that the texture of the Hering papers was furnishing the clue to discrimination. But Hering grays numbered one and two had been discriminated from yellow by one cat only with considerable difficulty and after a long period of training. We then sought to find a gray, of a texture similar to that of the yellow paper and of an intensity near to that of the grays mentioned above. We found a gray, an ordinary writing paper of rather dull finish, which fulfilled these conditions. We shall refer to it as Gray 1-2, since its flicker equivalent was mid-way between grays 1 and 2. This gray was confused with the yellow, as the following tables show.

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*Throughout the paper grays will be referred to as "colors" in this way.

[8] Washburn, M. F. *Psych. Bull.*, vol. 9, 1912, p. 54.

Though very extensive tests were made, we could find no gray paper which was not very promptly discriminated from blue. It occured to us that a very considerable range of gray cambrics is supplied by the dry goods stores. As a last resort we turned to trying these. Most of them were promptly discriminated from the blue, but finally a dark gray shade (F. E. —44-45) was found which neither Cat 3 nor Cat 5 could discriminate from the Bradley blue. Meanwhile, we had found a blue cambric which these cats confused with Bradley blue. For the sake of still further evidence we paired this blue cambric with the gray cambric. The two were, in turn, confused.

Again, in using violet as a stimulus color it was found to be confused only with a gray cambric of F. E.—48.

The following tables record the confusions of yellow, blue, red, green, and violet with grays.

Confusion of Yellow with Gray 1-2

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 2	275	325	600	46.8
Cat 4	300	300	600	50.0

TABLE V (A)

Confusion of Blue Paper with Gray Cambric

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 3	283	317	600	47.1
Cat 5	280	320	600	46.6

TABLE V (B)

Confusion of Blue Paper with Blue Cambric

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 3	287	313	600	47.8
Cat 5	280	320	600	46.6

TABLE V (C)

Confusion of Blue Cambric with Gray Cambric

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 3	285	315	600	47.5
Cat 5	274	326	600	45.6

TABLE VI

Confusion of Bradley Red with Bradley Black

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 8	273	327	600	45.1
Cat 3	271	329	600	45.1

TABLE VII

Confusion of Bradley Green with Dark Cool Gray*

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 4	272	328	600	45.3
Cat 9	301	299	600	50.11

TABLE VIII

Confusion of Bradley Violet and Gray Cambric. F.E.—48

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 8	278	322	600	46.3
Cat 9	292	308	600	48.6

TABLE IX

Summary of Confusions of Colors with Grays

Color	Gray Confused with the Color	F.E. of the Color
Yellow	1– 2	1– 2
Green	9–10	6
Violet	48	16–17
Blue	44–45	21
Red	50	25

The following curves show graphically the flicker-equivalents of the colors and the numbers of the grays with which they were confused. If Ives's results could be generalized to cover our flicker tests, these curves would represent the relative brightnesses of the colors for the cat and for the human eye.

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*This gray was somewhat faded. F.E.—9-10.

Thus far we have shown that, the colors, yellow, green, violet, blue, and red have each been confused with a colorless paper (or cambric) by the cats.

Each confusion was found to be identical for two cats.

In each case inability to discriminate was shown by six hundred trials, during which there was no increase in the proportion of right choices. (This is not shown in the above tables but it is shown clearly in our records by series of trials).

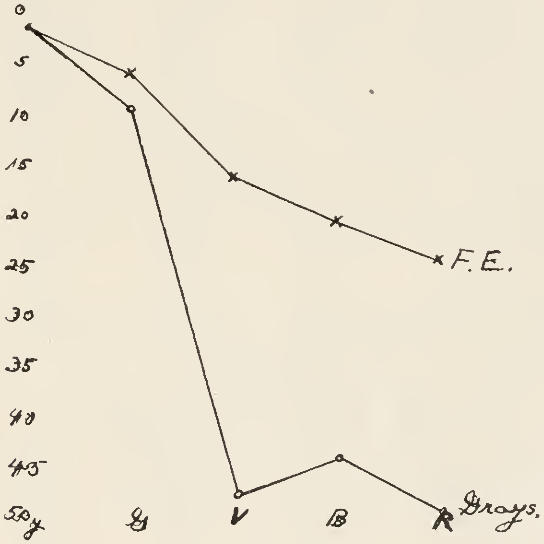

FIGURE 2

The confusions were made in each case after the cat had discriminated its food-color from a number of other grays.

The colors, blue, red, violet, and green were each confused with a gray which is much darker than the gray which represents the brightness value of that color for the human eye.

The yellow was confused with a gray of identical flicker equivalent.

Conclusions: It seems probable that cats cannot distinguish any one color from all the shades of gray, under light adaptation. It is possible that the animal may be totally color-blind by daylight.

Our results in the case of red seem to agree with those of other investigators, namely that red is of low stimulating value for the animals studied. But we may go further and say that blue, and violet are also of low stimulating power for the cat. This suggests the possibility of a much shortened spectrum (i. e. gray band). Yet blue is not confused with black by the cat.

CONFUSION OF COLORS WITH COLORS

Since our animals have confused colored papers with grays, even when the textures were as different as those of paper and cambric, they ought presumably to confuse many pairs of colors, all chosen from the same series of papers. To ascertain whether they would do so we tested each cat by presenting for its choice the stimulus color paired in turn with each of the eighty-nine remaining Bradley colors. In each case the behavior of one cat was confirmed by giving the same tests to a second one. As in the work with grays we began with yellow and blue as stimulus colors.

In the following tables we have included only cases of confusion and of difficult discrimination. The stimulus color was discriminated from all of the Bradley papers not named in the tables, and such discriminations were prompt, with but few exceptions. Examples are given in TABLE I (A). The confusions in each table are recorded in the order of their occurrence in the experiments, but it must be remembered that discriminations, both easy and difficult, intervened between the confusions in many cases.

TABLE X (A)

Confusions Made by Cat 2

Stimulus Color Yellow. F.E.—1-2

Yellow with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
GYT1	374	436	810	2	46.8
GYT2	274	326	600	1–2	45.6
YT1	286	314	600	1–2	47.6
YT2	267	333	600	1–2	44.5
OY	264	336	600	2	44.0
OYT1	276	324	600	2	46.0
OYT2	294	306	600	1–2	49.0
YO	236	364	600	4	39.3
YOT1	266	334	600	2	44.3
YOT2	293	307	600	1–2	46.8
OT1	230	280	510	3	45
OT2	298	182	480	2	60.2

TABLE X (B)

Confusions Made by Cat 4

Stimulus Color, Yellow. F.E.—1-2

Yellow with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
GYT1	207	393	600	2	34.5
GYT2	207	393	600	1–2	34.5
YT1	197	193	390	1–2	50.5
YT2	157	233	390	1–2	40.2
OY	172	128	300	2	54.4
OYT1	193	197	390	2	49.4
OYT2	293	427	720	1–2	40.7
YO	236	364	600	4	39.3
YOT1	266	334	600	2	44.5
YOT2	293	307	600	1–2	48.8
OT1	230	280	510	3	45
OT2	298	182	480	2	62.0

TABLE XI (A)

Confusions Made by Cat 3

Stimulus Color, Blue. F.E.—21

Blue with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
BT1	257	343	600	8	42.8
VB	238	362	600	21	39.6
BV	250	350	600	13	41.6
BVT1	209	391	600	7	34.8

TABLE XI (B)

Confusions Made by Cat 5

Stimulus Color, Blue. F.E.—21

Blue with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
BT1	260	340	600	8	43.3
VB	158	442	600	21	26.3*
BV	232	368	600	13	38.6
BVT1	199	401	600	7	33.1

TABLE XII (A)

Confusions Made by Cat 3

Stimulus Color, Red. F.E.—25

Red with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
RVS2	283	317	600	23	47.1
RVS1	210	390	600	20	35.0
ORS1	260	340	600	30	43.3
RS2	240	360	600	31	40.0
RS1	243	357	600	25–26	40.5
RT1	240	360	600	10	40.0
VRS2	260	340	600	30	43.3
VRS1	230	370	600	23	38.3
VR	257	343	600	17	42.8

---

*It will be seen from this record of VB that an animal may make as high as 73% of correct choices and yet fail to discriminate twenty-four times out of thirty trials, and *this failure is confirmed by the record of Cat* 3 *on the same color.*

TABLE XII (B)

Confusions Made by Cat 8

Stimulus Color, Red. F.E.—25

Red with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent or Error
RVS2	277	323	600	23	46.1
RVS1	219	381	600	20	36.5
ORS2	258	342	600	30	43.0
RS2	247	353	600	31	41.1
RS1	240	360	600	25–26	40.0
RT1	238	362	600	10	39.6
VRS2	257	343	600	30	42.8
VRS1	238	362	600	23	39.6
VR	241	359	600	17	40.1

TABLE XIII (A)

Confusions Made by Cat 9

Stimulus Color, Green. F.E.—6

Green with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
YGS1	230	370	600	7	38.3
YG	241	359	600	3	40.1
YGT1	239	361	600	3	39.8
GS2	290	310	600	8	48.3
GS1	257	343	600	7	42.8
GT1	237	363	600	3	39.5
BG	288	312	600	7	44.6
BGT1	240	370	600	3	40.0
GBT1	261	339	600	6	43.5

TABLE XIII (B)

Confusions Made by Cat 4

Stimulus Color, Green. F.E.—6

Green with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
YGS1	260	340	600	7	43.3
YG	239	361	600	3	39.6
YGT1	257	343	600	3	42.8
GS2	257	343	600	8	42.8
GS1	230	370	600	7	38.3
GT1	240	360	600	3	40.0
BG	238	372	600	7	39.3
BGT1	290	310	600	3	48.3
GBT1	253	347	600	6	42.1

In all, these tables show a total of thirty-four confusions of a color with a color. This strengthens the probability suggested by the experiments with grays, that wave length may not be a stimulus for the cat. It is further strengthened by the close agreement of the flicker values of the yellow, that of the gray confused with that color, and that of the colors confused with yellow. The gray, the yellow itself and five of the colors confused with it have the same flicker-equivalent, namely 1-2, while the six remaining confusion colors differ very slightly from that number. Other groups of colors differ widely from the flicker values of the stimuli, which agrees once more with the experiments with grays.

The relations of the flicker values of the colors to those of the stimulus colors are shown in Figure 3.

The cats have been found to confuse each stimulus color with a certain gray on the one hand, and with a group of colors on the other. If we should assume that in each pair of these papers the cat has merely seen that gray once more, we may assign to the group the brightness value of the gray with which the stimulus color was confused. The gray values are then represented, Figure 4, by the straight horizontal lines, whose positions probably indicate the "brightness equivalents" of the several colors for the cats, in the sense in which Miss Washburn uses that term (pp. 145 and 146).

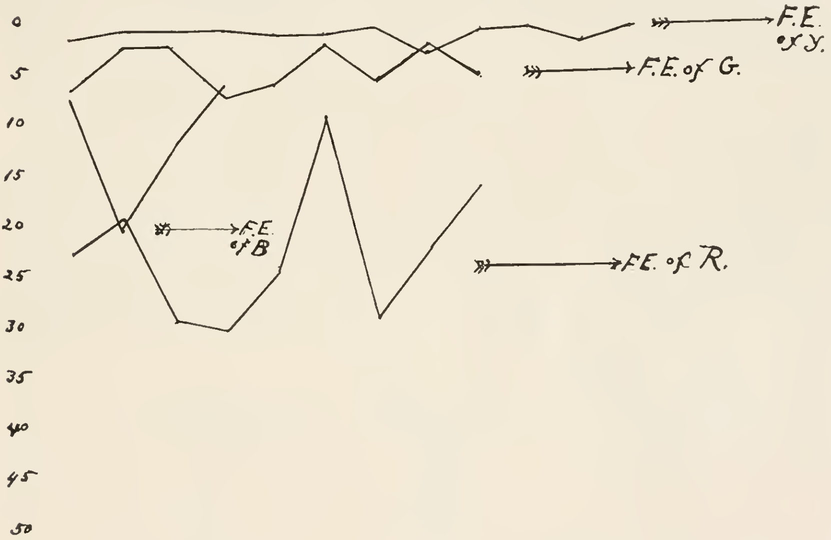

FIGURE 3

The deviations of the curves from the horizontal then indicate how much the several colors may vary, for the human eye, from that gray and yet be indistinguishable from it for the animals. These deviations are large for red and blue, slight for green, and very slight for yellow, as if there the animals approximated the brightness-difference-threshold of the human eye.

The deviations probably show much more the defects of colored papers. They are complex colors and while one factor, say the tint, may presumably brighten the paper, another, say the red or violet, darkens by at least an equal amount. Thus RT1 is very much brighter than R for the human eye, yet the influence of the tint is slight in comparison with the influence of the red for the animal. Hence it is still confused with red. RT2, however, is discriminated from R after many trials, but darkened orange-red is confused. The relations are shown in the following tabulation.

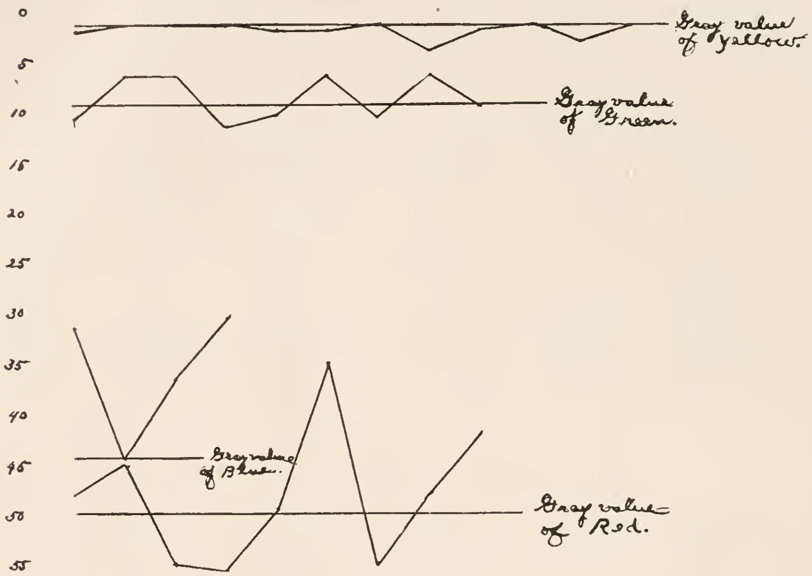

FIGURE 4

Red with	Errors	Correct Choices	No. of Trials	F.E.	Verdict*
RT1	238	342	600	10	Confusion
RT2	156	444	600	7	Diff. Dis.
ORS2	258	342	600	30	Confusion
ORS1	240	360	600	30	Diff. Dis.
OR	6	24	30	15	Discrimination

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*Based on our standard of twenty-four correct choices out of thirty-trials, as the test for discrimination.

This low stimulating effect of red has been shown for the dancing mouse, the rabbit, and "possibly"[9] for the monkey. Apparently the cat is no exception to the rule. Yet even in the case of red the cat appears to need no such enormous differences in brightness as does the dancing mouse, in order to discriminate promptly. Nevertheless it would be quite unfair to base an opinion of the cat's discriminating ability on his reactions to red.

DISCRIMINATIONS

Our account of the confusions made by the cats is complete. Only matters of minor importance are shown by the discriminations. To our surprise no trace of individual variations appeared in the confusions. This is more apparent because of our determination to employ so many trials that the results would not be vitiated by improvement due to training.[10]

Individual differences did come to light in what we have called difficult discriminations, i. e., those which required at least two hundred forty trials for the animal to learn to discriminate. The greatest difference between any two animals appears in certain colors presented with yellow as the stimulus color. It is indicated by the number of trials required for discrimination by each animal and is shown in the following table.

TABLE XIV

Yellow with	No. of Trials for Cat 2	No. of Trials for Cat 4
G	240	30
GT1	330	30
GT2	360	60
YG	330	30
OYS1	60	330
O	630	630
ROS1	240	240
RO	480	480
VRT2	210	150

There are four prompt discriminations by one cat, only one by the other. The great difficulty of discriminating orange from yellow shown by both animals indicates that it must appear to them very much like the yellow. In this case stopping after five hundred seventy trials would have resulted in a decision that orange is confused with yellow. The per cent of right choices of orange was but 66.2, of VRT2 by Cat 4, only 60.7.

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[9] Watson, J. B. Some experiments bearing on the color vision of monkeys. *Jour. Comp. Neur. and Psych.*, vol. 19, 1909, p. 19.

[10] See Yerkes, The Dancing Mouse, pp. 127 and 128.

There were six cases of difficult discrimination with blue, four with red none with green.

It remains to present the colors which, in experiments with yellow as a stimulus color, were promptly discriminated from it and yet were of nearly the same flicker value as the yellow. It will be interesting to set down in parallel series the colors confused with yellow, those discriminated from it with difficulty, and those easily discriminated from it but of similar flicker value, and underneath each color the number denoting its flicker equivalent.

Stimulus Color, Yellow. F.E.—1-2

1. Confused

GYT1,	GYT2,	YT1,	YT2,	OY,	OYT1,	OYT2,	YO,	YOT1,	YOT2,	OT1,	OT2
2	1–2	1–2	1–2	2	2	1–2	4	2	1–2	3	2

2. Discriminated with Difficulty

G,	GT1,	GT2,	YG,	OYS1,	O,	ROS1,	RO,	VRT2
6	3	1–2	3	5	6	15	9	5

3. Discriminated Easily

RVT2,	VT2,	BT2,	GBT2,	BGT1,	BGT2,	YGT1,	YGT2,	GY,	ROT2,	ORT2,
2	3	3	2	3	2	3	1–2	3	2	3

The first two series of flicker-equivalents show that flicker values were a factor in producing confusions and in causing difficulty of discrimination. In the second series the presence of orange, which we have already seen to be almost as bright as yellow for the cat, is a factor which produces difficulty in four cases. In the case of GT2 the extreme brightness of the tint is overcome by the darkening effect of the green, though they come near balancing each other.

In the third series of flicker values the effect of red, violet, blue, green, and orange in darkening the tints for the cat is very evident. This would indicate that those colors have great darkening effect and that fact confirms to a great degree the brightness position we have given them in Figure 2, which is drawn from the results of grays.

If further evidence of the opposite effects of antagonistic factors is needed, we may take the case of GY. Though the flicker value differs slightly from yellow the presence of green enables discrimination. Add a "tint" and confusion takes place. Brighten the color to the next brighter tint and the result is still confusion. Thus,

Color	F.E.	Result
GY	3	Discrimination
GYT1	2	Confusion
GYT2	1–2	Confusion

Here the effect of the tints overbalances the effect of the green and produces confusion. Elsewhere the darkening effect of the colors is so enormous that the tints and shades have relatively slight effect on the cat. This was to be expected, of course, if the curve derived from experiments with grays is approximately correct. Altogether the results derived from experiments with grays and with colors are surprisingly consistent.

USEFULNESS OF FLICKER EQUIVALENTS

The great drawback of our experiments was the time they consumed in trying so long a list of colors. Could this, by any means have been shortened ? In our work with colors had we tested for yellow only those of its flicker equivalent, we should have discovered five of the twelve confusions. Had we explored one-half of a flicker unit on each side the yellow, we should have discovered five more confusions, and had we explored for two and one-half units on either side, we should have found them all. To have done the same thing with blue we should have had to explore a range of fourteen units, but using only the exact flicker equivalent would have found for us one of the four confusions with blue.

Exploring a range of three flicker units on either side the green would have discovered all the confusions. Using the exact flicker value of the green would have revealed one confusion. Using the *exact* flicker-equivalent of red would not have yielded any one of the confusions, (It is true only of red.) but using one half of a flicker unit on either side of the red would have brought out one confusion. Two flicker units would have shown three of the nine confusions, five units would have shown six, fifteen flicker units would have shown them all.

Even exploring a range of fifteen flicker units on each side of every stimulus color would be a very different affair from pairing each stimulus color with each of the eighty-nine other Bradley colors. Such procedure in the experiments with these cats would probably have reduced the time required from twenty-eight months to eight. An animal which discriminates the stimulus colors from all those of the same and nearly the same flicker values has a very different type of vision from that of the cat.

Though the colors have vastly different brightness values for the cat and for the human eye, the brightness equivalents and the flicker-equivalents have intersected many times. A claim that the use of flicker values would not save time must assume that four broad bands of flicker-equivalents on either side of yellow, blue, red, and green respectively might none of them meet, at any point, the brightness values of the animals. Such an assumption is so improbable that in work with colors the neighborhood of the flicker-values of the stimulus colors should be explored first, provided the animal has already made a number of discriminations to become accustomed to the experiment.

In the experiments with grays, yellow was confused with a gray of the same flicker value. To find the confusion gray for green, we should have had to use a range of four flicker units. With red, blue, and violet flicker values would have been useless for discovering the grays, and "systematic groping" such as we have used in our experiments would be necessary in finding the gray values of those colors for the cats.

GENERAL REMARKS

We frequently tried the cats with two glasses lined with the same colored paper, e. g., two yellows, two blues, etc. They failed, so that the assumption that they distinguished by wrinkles or spots on the paper is gratuitous.

Our records show practically a dead level of uniformity in the responses of each pair of cats. It seems hardly necessary to confirm the work of one cat by giving the same tests to another. It adds a trifle of reliability, but it has added but one new fact to our results, and that of slight importance.

This uniformity of behavior suggests also a dead level of stupidity. A glimmer of intelligence was observed, for, as already stated, one cat gave good evidence of selecting the food-glass by the position of the thumb-button at the rear of the apparatus, and as good evidence of failing to do so when the buttons were concealed by shields.

Our experiments show that the cat has very defective daylight vision as compared with that of human beings. Is it possible that this defective vision accounts for the behavior of Thorndike's cats which clawed at the place where the loop had been when the loop was no longer there ? For such vision as the cat possesses, a mad scramble would conceivably be a much quicker way to lay hold of a loop than an attempt to see it. An accident of similarity of brightness between the loop and the background might render it well nigh invisible to the animal. Is it possible that the poorer the vision an animal possesses the more he becomes dependent on kinaesthetic sensations, which Watson has shown to play a fundamental role in the life of some animals.

Our records show that an animal may make more than fifty per cent of right choices throughout a large number of trials and yet not learn to discriminate between the two objects.

Our experience shows that the possibility of the texture error should be guarded against, as well as the error due to improvement by training. In some cases discrimination occurred only after eight hundred trials.

So many criticisms have been made of the use of colored papers that one advantage in using them, no matter how trifling it be, should be welcome. All the confusions made by these cats can be exhibited to the eye by pasting the papers on gray cardboard. The result of viewing the papers will be a better conception of the nature of the cats' vision than can be got from reading pages of description of their behavior in the experiments.

Finally we asked two persons of dichromatic vision to sort these colored papers as Holmgren worsteds are sorted. Each of the dichromates made five confusions which had been made by the cats. Both of the dichromates and the cats agreed in the matches (confusions) of two pairs of colors, and for each of these pairs the flicker-equivalents were identical.

Our account of our exploratory tests of the cats' vision is finished. We hope that feline vision may now be studied quantitatively, by means of apparatus which permits of accurate measurement of the wave-lengths and intensities of the lights, as they reach the eye of the animal.

THE WHITE RAT AND THE MAZE PROBLEM

II. THE INTRODUCTION OF AN OLFACTORY CONTROL[1]

STELLA B. VINCENT
Chicago Normal College

What part has olfaction in the life of a rat? The answer to this query would have to be based upon what we know of brain structure and from our casual observation of rat behavior, since, there has been very little direct experimentation published that has as its main concern this form of sensitivity.

The rat has well defined olfactory lobes and tracts. But these parts are relatively smaller than those of some other rodents and decidedly smaller than those of some other mammals. The olfactory paths in the brain of the rat have not had much study and we are thrown back, therefore, upon what we know of the life and habits of the animal for the answer to our question.

It might be thought, from watching the reactions of the rats in the maze, that smell was a very important sense. The frequent sight of a rat lifting itself on its hind feet and sniffing vigorously, the constant use which it makes of its nose on the floor and sides of the maze, would lend credence to such a supposition. Yet it has been shown that anosmic animals are under no serious disadvantage in learning the maze and that much of this sniffing and apparent smelling has an important tactual function. What the world of odor is to a rat we have little power of conceiving but how it affects the behavior we may somewhat discover.

The odors which are vital in the animal world are, presumably, food odors, sex odors and body odors. By the term body odor is meant those olfactory qualities which perhaps are peculiar to individual animals but which certainly characterize the animals of a single cage or group. By differentiation from this familiar odor it serves to mark off a strange animal or give warning of an enemy.

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[1] This work was done in the Psychological Laboratory of the University of Chicago. I am greatly indebted to the department for the opportunity to do it and to professor Carr for suggestive help and criticism of both experimentation and paper.

Rats are omnivorous and hence there can be slight necessity for any fine discrimination in the way of foods. A generalized response to food odor will be all sufficient. I have, indeed, never seen in white rats any clear discrimination of foods which might be said to depend upon smell and have failed to find any mention of such power by others. If food be introduced into a cage unobtrusively, a rat usually stumbles over it before discovering it. It might be supposed that blind and normal rats would show different behavior in food seeking, yet in some preliminary experiments covering several weeks, the food in every instance, by both normal and blind rats, was apparently found accidentally. The animals were very tame and were very hungry. The food used was nuts, cheese and milk soaked bread. The experiments, although significant, were too brief to be conclusive. The instances which Small[2] cites of the reactions of very young animals to different odors may clearly depend upon the chemical sensitivity of the mucus membrane of the nostrils and must be sharply distinguished from olfaction proper. Professor Watson,[3] however, found that blind animals, otherwise normal, were affected by odors to which anosmic animals failed to respond. To repeat, smell is more closely associated with food getting than is any other sense; yet it may be safely assumed, and we should expect to find, that the sense is less refined in animals which do not pick and choose their food than in those which do.

If a rat from another group is introduced into a cage containing other rats they "nose" the whole body of the stranger. The rats do not appear to get the odor across the cage for the excitement and characteristic actions begin only with contact. Rats also respond by different behavior to strange handling. No doubt a large part of the excitement is due to different methods of lifting, etc.; but after the emotional disturbance is allayed the "nosing" of the hand seems to indicate an odor stimulation also. The power to follow a trail is usually supposed to depend upon slight traces of body odor which remain upon the path which an animal has taken. Animals which do not prey upon others for food have little need for tracking. Experimentation has failed to show such ability in these animals.

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[2] Small, W. S. Notes on the psychic development of the young white rat. *Am. Jour. of Psych.*, **11**, 89.

[3] Watson, J. B. Kinaesthetic and organic sensations, etc. *Psych. Rev. Mon. Sup.*, **8**, no. 2, p. 65.

Sex odor calls forth specific behavior. This odor, however, does not seem to carry from cage to cage even though the cages are placed side by side. Efforts to establish the tracking of one sex by the other have been made[4]. Watson said he found no good evidence of tracking but that adult rats showed preferences for entrances that contained the odor of the opposite sex.[5] Small insists that he had no evidence to show that the males followed their own tracks or those of other males or that females followed the tracks of the males.[6] Possibly these attempts have not been made at the right periods; at least the results are inconclusive.

How well rats or other animals can localize odors is still an open experimental field as is also the possibility of olfaction functioning in giving distance values.

The object of this work was to see whether an olfactory control could be introduced into the learning of the maze, and, if it could be, to discover how it would affect the learning process as compared with other forms of control.

The modified Hampton Court maze was used, the same one which served for the experiments with vision.[7] Before beginning the work, the inside of the maze was heavily coated with white enamel paint to cover and to destroy any previous odors, and upon the floor of all of the runways were laid long strips of heavy white paper. The paper was cut 4 in. in width and where the strips overlapped they were fastened with gummed paper. Upon this papered floor was rubbed in, down the center of the runways, a narrow trail of alternating beef extract and cream cheese. It was thought better to use two substances in order to guard against a possible olfactory fatigue. The trail was laid upon paper because of the ease with which such a covering could be removed in varying the experiment and because of a desire to avoid a permanent odor in the maze.

The rats used in this work were young, untrained rats about

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[4] Watson, J. B. Animal Education, p. 51.
Small, W. S. Experimental study of the mental processes of the rat. *Am. Jour. of Psych.*, **12**, 232.

[5] Op. cit., p. 53.

[6] Op. cit., p. 213.

[7] Vincent, S. B. Vision in the maze. *Jour. Animal Behav.*, **5**, 1.

90 days old. They were fed in the maze and handled for a week preceding the beginning of the real work. During the experimentation they ran the maze three times a day under the stimulus of hunger and were amply fed at the conclusion of each day's work. The first experiment was one in which the trail was laid in the true path in the maze and not in the *cul de sacs*.

EXPERIMENT I. Olfactory Trail in True Path

1. Behavior

The behavior in this experiment will be described somewhat in detail since it is significant. There was none of the wild running seen in the usual maze reaction. When put in the box the rats were at once attracted by the odor. Their little noses went down to the trail and they began to follow it immediately. They moved along in a jerky fashion stopping occasionally to smell and to lap the trail with their tongues. This manner of running made their progress an exceedingly slow one. Both the cheese and the beef extract which were used were diluted with water so that there was but a very slight trace of the food on the paper. Still the animals may have obtained some satisfaction in lapping, but such gratification must have been very limited. In general the rats lingered longer over the cheese than over the beef extract trail. The odor was probably stronger. They often hesitated at the places where the trail changed from one substance to another and sometimes struck the "back" or "home trail" here. These returns only now and then resulted in an entrance into a blind alley. They usually ended where the trail changed again. The maze is so constructed that the food box is in the center. When in use, there is always food in this box which the animals are encouraged to smell before the beginning of the experiment and which furnishes their reward when they reach the box at the end of their run. The true path passes directly by the side of this box. (See "Vision in the Maze," Fig. 1.) In the normal maze the early runs are always broken at the food box which the animals have to pass in the center of the maze. The food odor is stronger here and they bite and claw and scratch in a futile endeavor to end the quest at this spot. But notwithstanding the marked early influence of the odor of the food box this behavior, in the normal maze, is very quickly abandoned.

Long before the rats cease to enter the *cul de sacs*, before any of these errors are entirely cut out, the loitering at the food box is no longer to be seen. It only thereafter occurs in exceptional cases where an animal is entirely lost and as a consequence is in a disturbed and emotional condition in which all the old errors reappear. The behavior of the animals following the odor trail, on the contrary, although similar at the food box was more persistent than any "off trail," blind alley error. The odor, it will be remembered, was that of the food with which they were accustomed to be fed. Perhaps the previous stimulation of the olfactory trail had made the animals more susceptible to this influence. But whether, as a result of following a food odor trail, all food odors attracted the attention more, or whether this stronger food odor represented the natural instinctive ending of a food trail and thus called a halt, these are questions for thought. Either or both positions are plausible.

Whatever the cause of this behavior, as a result of it, the speed in all of the early trials was slower than that in the normal maze; but by following the trail the animals were kept in the true path so that the errors were greatly decreased in both the initial and in the succeeding trials.

2. The Tables

Table 1 shows, side by side, the records for the first twenty-five trials in the normal and the olfactory mazes. Figs. 1, 2, and 3, show the curves plotted from these records. These curves are not made like those shown in "Vision in the Maze" because in the olfactory maze the learning period covered less than ten trials and was practically uniform. The units used in plotting were one trial, one minute and one error. Since it was the following of the trail in which we were interested, the error consisted in leaving the track. Returns were not counted and this fact makes these curves comparable with those made for the black-white maze where the returns could not be counted.

The results of this experiment show an increase in accuracy, both initial and total, over the normal maze and an increased final speed. We will consider first the facts which bear out these assertions as to accuracy.

3. Comparative Accuracy

As the table shows, in the first trial, these animals in the olfactory maze averaged only 4.5 errors as compared with 14.7 made in the normal maze. Thus the initial accuracy was three times as great. The final accuracy was greater also. The olfactory maze shows .04 average errors per trial for the last five trials while the normal maze has an average error of .1 per trial for the same five runs. The total number of errors per animal in the olfactory maze is only one-third that of the animals in the normal maze. The error curve, seen in Fig. 1, bears out all of the above statements. Its chief features are the low beginning height, and hence slight fall, and the almost complete low level which it maintains after the twelfth trial. A comparison of the error curve of the normal maze with this will emphasize these facts better than words.

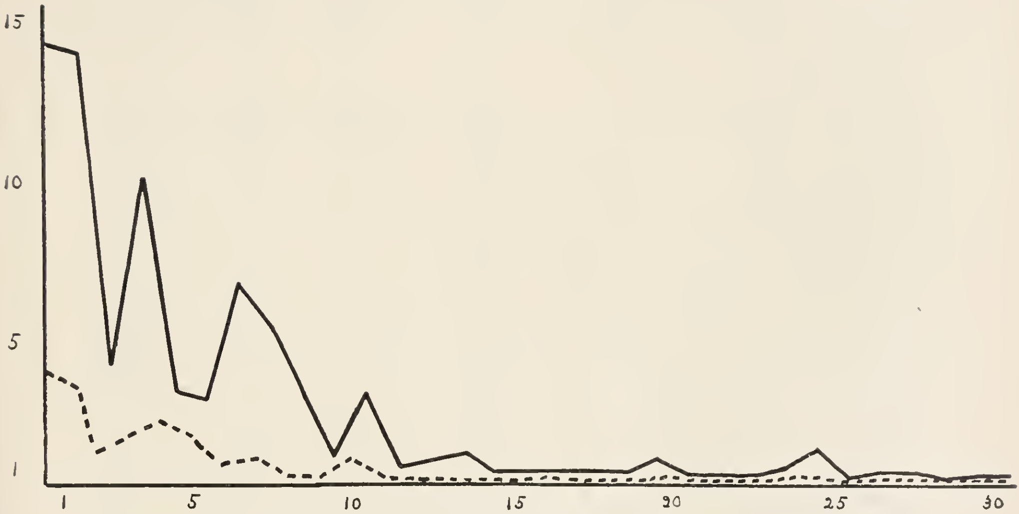

Figure 1. Time and error curves for Experiment I. Olfactory trail in the true path. Full line time, dotted line errors.

4. Speed

The time per run for the early trials was less than in the normal maze as may be seen from the table but this was entirely owing to the fact that there were so few errors. The actual speed was much slower. In the first trial they averaged only 4.5 errors per animal yet the time average is 13.5 minutes. The record for the second trial is practically the same. Almost the same average number of errors, 4.1, was made by the normal animals in the fifth trial in an astonishingly shorter time. For the first trip without error these rats had an average time of 160 sec. The time record for fifteen rats in the normal maze for the first perfect trip is less than one-fifth of this—30 sec. In final speed, however, these animals excel. This maze has an average record of .28 min. for the last five trials as against .31 min. for the normal maze. This is a difference of nearly two seconds—an appreciable difference when one remembers that the maze can be run in ten seconds.

The time curve (Fig. 1) is very unlike the usual time curve. Compare it with Fig. 3. It is not the beginning height which is remarkable but the persistence with which it maintains this level—the slow rate of elimination of the surplus time. Forty-seven per cent of the surplus time was eliminated in the normal maze in the second trial, in the olfactory maze only 2.5% was eliminated at this time; 80% was eliminated in the first four trials in the normal maze, but it took the rats in the olfactory maze nine trials to reach this point. By the tenth trial the animals in the normal maze had only 2% surplus time left to eliminate, but the rats in the olfactory maze did not fall permanently below this 2% point until the twenty-fifth run.

It must be clearly evident that this olfactory trail was affecting the learning process but before any definite conclusions were drawn it was necessary to put the trail in the *cul de sacs* instead of the true path and to see what would happen then.

EXPERIMENT II. TRAIL IN *CUL DE SACS*

1. BEHAVIOR

This experiment was conducted exactly like Experiment 1, with animals of the same age, etc. The only difference was in the trail which was laid from the entrance of each *cul de sac* to its extreme end. There was a noticeable difference in the numerical results as well as in the behavior under these conditions.

The animals in this maze also made fewer errors from the beginning than the animals in the normal maze and the speed was greater also. When put in the maze the rat ran, as in the usual maze, headlong down the runways. Soon he blundered

TABLE I

Records of the First 25 Trials, Time and Errors, of Rats in Normal and Olfactory Mazes

Trial	Average Time in Seconds per Trial			Average Errors per Trial		
	Normal	Olfactory trail in true path	Olfactory trail in errors	Normal	Olfactory trail in true path	Olfactory trail in errors
1	1804	820	991	14.7	4.5	9.6
2	966	800	463	11.9	4.	5.6
3	1043	224	598	10.4	1.1	7.3
4	847	609	331	7.4	1.6	5.3
5	231	175	49	4.1	2.	2.
6	192	165	54	3.5	1.5	1.8
7	64	376	30	1.6	.6	1.1
8	49	295	27	1.4	.8	.5
9	37	178	37	1.5	.3	.5
10	32	52	22	1.1	.3	.1
11	26	155	30	.7	.6	.1
12	25	29	36	.4	.3	.3
13	31	35	32	1.	.1	.1
14	20	52	21	.3	.8	0.
15	32	27	45	.6	.3	0.
16	46	26	83	.7	.1	1.1
17	44	24	97	.5	0.	.3
18	51	25	173	.6	0.	2.1
19	40	23	150	.1	0.	.8
20	32	39	177	.2	.1	1.1
21	31	29	94	.2	.1	.7
22	26	32	262	0.	.3	1.7
23	17	32	117	0.	.3	.8
24	19	37	107	.1	0.	.8
25	22	76	147	0.	0.	.7

TABLE II

Tabulated Statement of the Results in the Three Mazes

	Normal Maze	Olfactory trail in true path	Olfactory trail in errors
Average time of learning	12.1 ±3.6 trials	8.1 ±2.4 trials	7.3 ±3.8 trials
Average time of the first five trials	16.3 ±6.7 min.	8.7 ±3.9 min.	8.1 ±5.2 min.
Average speed of the last five trials	.31 ± .05 min.	.28 ± .08 min.	.47 ±.08 min.
Total surplus time	93.9 min.	64.98 min.	66.45 min.
Average errors first trial	14.7 ±7.7	4.5 ±3	9.6 ±6.8
Average errors in the last five trials	.1 ± .14	.04 ±.04	.44 ± .21
Total average errors per animal	66.6 ±16	20.5 ±5.6	52.1 ±12
First run without error	8.3 ±3.1	6.3 ±2.8	7.5 ±1.8

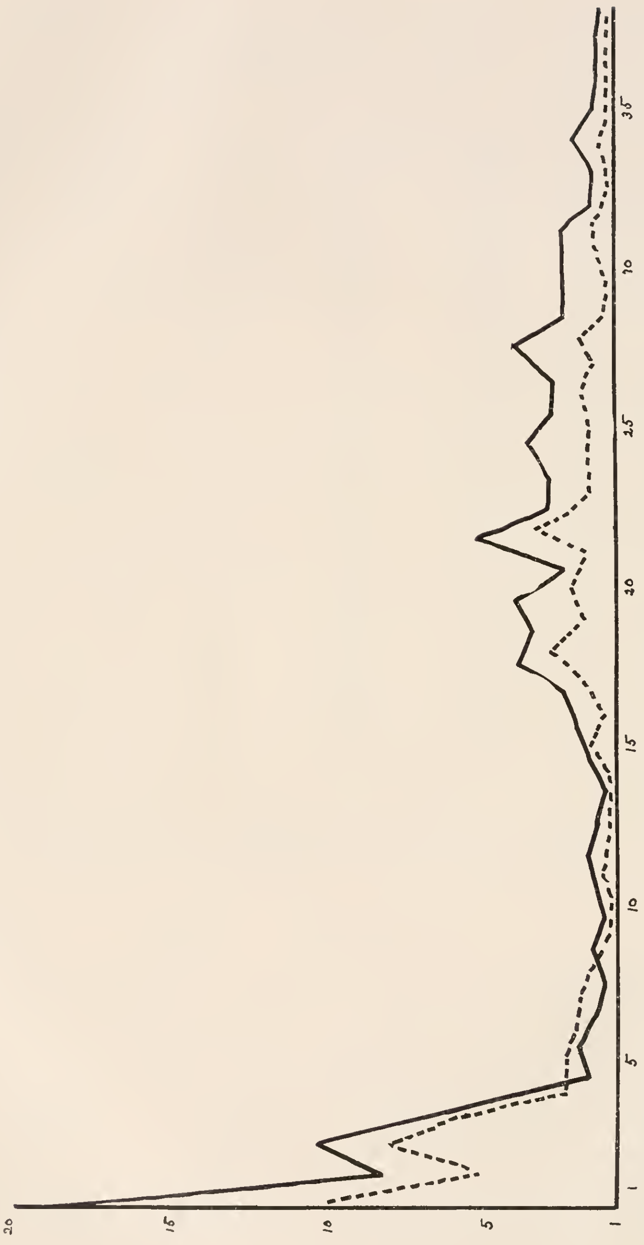

FIGURE 2. Time and error curves for Experiment II. Olfactory trail in the *cul de sacs*. Full line time, dotted line errors

into a *cul de sac* and down went his nose to the trail which he followed for its entire course, to the end of the alley. He moved along by jerks, as described before, and when he reached the end, he turned and in the same irregular, slow, halting way returned to the entrance of the alley. Between the *cul de sacs,* he ran; but when in them, slow movements were the rule. As a result more time was spent in a single *cul de sac* than had been the case in any of the other experiments. Still, from the first, these excursions from the true path were lessened in number as compared with the normal maze. The blind alleys seemed to be marked for the animal in some way. He began to go less and less deeply into them and finally, as he was running more and more confidently in the true path, I have seen him, time and again, actually thrown back on his haunches if chance running flung him into the entrance of a *cul de sac.* Or, he might be running quickly, swerve into an entrance, and there would be seen an instant decisive turning the minute he struck the trail. It looked like a real discrimination. Surprisingly enough, however, after the problem was learned, and the animal was making 90% correct trials, these errors began to reappear and it took almost as long to get rid of them the second time as it did the first. The meaning of this will be discussed later. There were many more returns in this experiment than there were in the one where the trail was laid in the true path—five times as many in the first trial, It was a long time before the rats learned to pass the food-box without lingering. The numerical results for accuracy confirmed the conclusions drawn from the observed behavior.

2. Comparative Accuracy

Under the conditions of this experiment, the accuracy was decidedly greater than in the normal maze in the first fifteen trials. If we now make a comparison with the other olfactory experiment, we find that more errors were made in the first nine trials than were made by the animals which followed the trail in the true path but that the next six trials were more perfect. From the fifteenth trial on, the accuracy was far less than in Experiment 1, or in the normal maze, and it was only toward the end of the experiment, that it again approached their standard. The curve (Fig. 2) shows this variation exactly. The total average number of errors per animal was 20% less than in the normal maze but the animals made two and one-half times as many errors as their brothers in the experiment where the trail was in the true path. The learning time was actually shorter than in Experiment 1. There is so little difference, however, that it may be a matter of chance. The conditions, as a whole, were very favorable for learning as compared with the normal maze and scarcely less so than those in Experiment 1, where the trail was in the true path. It seems fair to conclude, therefore, that these conditions did affect the accuracy, and in general favorably, but that there was a variableness in the final reactions which will require explanation.

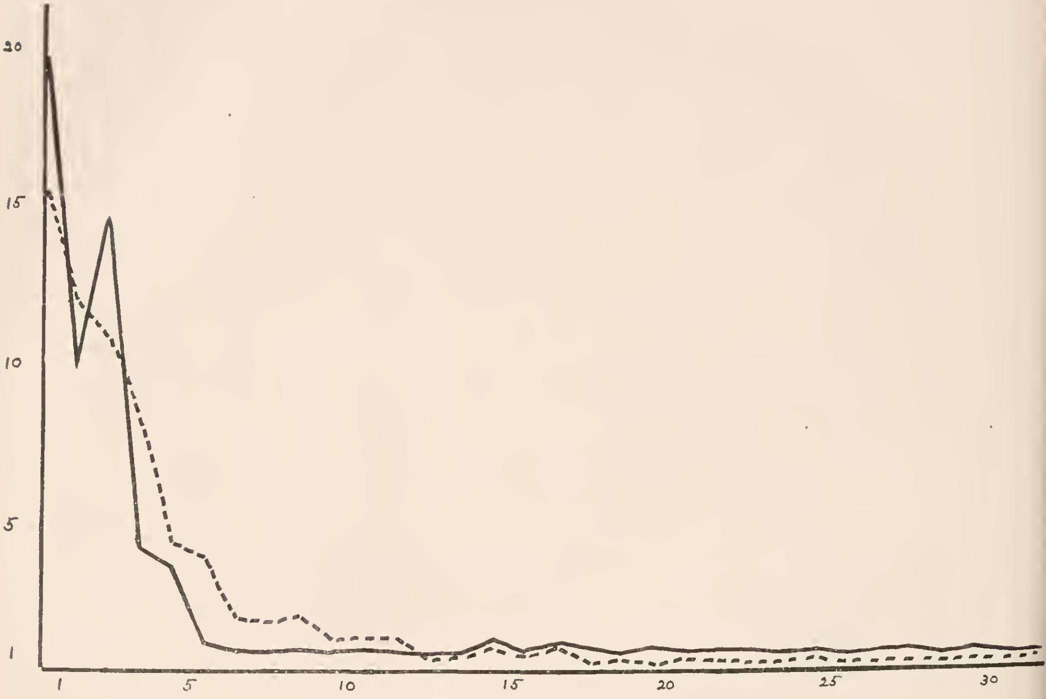

FIGURE 3. Time and error curves for normal maze. Full line time, dotted line errors.

3. COMPARATIVE SPEED

The speed in this maze was quite comparable with that in the normal maze except when the rats were in the *cul de sacs* and, because the errors were so few, the slowness in these places placed the animals under only a slight disadvantage. The running was much more rapid than that reported in Experiment 1. The figures in Table 1, giving the time per trial, do not show this since the data for the total distance is lacking. In the first trial in Experiment 1, there was an average of 4.5 errors and 1 return. The animals did not go to the end of each *cul de sac* and the returns were only partial. In this experiment, with trail in *cul de sacs* in the first trial, there was an average of 9.6 errors and 5 returns per animal. The larger proportion of these returns were home returns and the *cul de sacs* were explored to their farthest limits. According to these figures the time should have been three times as long in the latter case had the speed been comparable, instead of which it is practically the same (See Table 1). From this we should conclude that the speed was three times as great in the first trial in Experiment II as it was in the same trial in Experiment 1 where the trail was in the true path. There was a variability of speed in the middle part of the experiment which clearly depends upon the increase in errors. (See the curves Fig. 2). The average speed of the first trial without error may be taken as a point of comparison as we do not possess the figures for the total distance. The normal maze gives us an average of 30 sec. for this trial, the maze with the trail in the true path 160 sec., and this one 28 sec. At this point, then, in this experiment, we have a speed which is quite as fast as that in the normal maze. The final speed, however, within the limits of the experiment, was less—.47 min. (See Table 2). Whether longer experimentation would have developed a speed equal to that in the normal maze, or whether the conditions would always have mediated against it is a question for discussion.

EXPERIMENT III. TRANSFER OF TRAINING

This experiment was the crucial one. The conduct of the rats had been affected by the olfactory trail in the maze, the learning had been aided, but had the animals really gained anything which they could carry over to another problem ? Was an olfactory control so well established that it could be utilized in another situation ? It was determined to take the animals, at the conclusion of Experiment 1 on the maze, over to a problem box. This box had three runways, leading from a common entrance, and they terminated in a food-box. The paper trail, some of the original paper from the runways, could be laid along these runways, changed in irregular order, and the rats tested here. Before taking them to the box, however, after the conclusion of Experiment 1, the paper was entirely removed from the maze and the rats given one trial each on the maze itself. They made perfect runs showing no hesitation whatever. They did not seem to miss the paper at all and even incorporated in the runs a slight "slowing up," as had always been the case at the places where the trail changed from beef extract to cheese. Evidently the control had become kinaesthetic. The question we had to face now was this: Had the olfactory experience persisted notwithstanding the change of control.

The rats were now taken over to the box. The first trials here gave entirely negative evidence. The olfactory trail might as well have been absent for all the attention which the rats gave to it. The path with the trail was only taken on an average of six times out of twenty trials. The next morning the animals were tried again and then it was seen what they were doing. No matter where the trail was laid, they were always making a straight run to the left and down the runway on the left side. Now this was just their first run in the maze. Clearly kinaesthesis was at the helm and olfaction had retired from the engagement. It was necessary, therefore, to arrange conditions such that the opportunity to make this run to the right or to the left should be done away with—a condition in which *position* so far as possible should be eliminated.

A long rose box, about three feet in length was procured from a florist and in its end were inserted long, heavy, pasteboard mailing tubes. These tubes just filled one end of the box. They were lined with paper taken from the maze and one tube contained paper on which was the trail. In the experiment the tubes were alternated according to an irregular schedule. For the next few days the rats were tried out in this box. When they were put in at the end farthest from the tubes they immediately ran down to these exits. The two openings were side by side, there was no chance to turn, and in fifty trials they made 90% correct choices: i. e., they followed the trail nine-tenths of the time. While sitting in front of the tubes the rats could smell either one indifferently so there was usually a momentary hesitation at the entrances and then a dash into one or the other. Sometimes the head was put in tentatively and then came the sudden run through or the withdrawal. The experiment showed, conclusively, that the olfactory experience had been retained and that it could be utilized again. It also showed that the reaction to the original problem had become a matter of habit and that so strong and powerful was kinaesthesis that the removal of the sensory factors which helped to establish it had no effect upon its control. When later the animals were confronted with a problem where turning to the right or to the left was possible the response was in kinaesthetic, or tactual-motor terms. But when the possibility of runs and turns were cut out the effects of the olfactory learning and experience were asserted in a perfectly effectual way. That this was not due to any attractiveness of the trail in itself is shown by Experiment IV.

EXPERIMENT IV. ANOTHER TRANSFER

This was the discrimination test for Experiment II. The same box and the same method was used as in Experiment III. Under these conditions the animals had to choose the path where there was no trail. They did this just as consistently as the others making just as good a record and confirmed in all points the conclusions drawn from Experiment III. The details are not needed here.

DISCUSSION AND CONCLUSIONS

Whatever may be true of rats in their native environment, we agree with Small,[8] that these animals do not usually follow a path in the maze by means of scent; yet, as these results show, they can do so. The evidence here is also against Professor Watson's statement that. "Olfactory sensations have no rôle in the selection of the proper turns in the maze.[9]" This assertion may be quite true of work on the maze as he used it, but certainly olfaction, in the experiments reported in this paper, helped to cut out the errors. Although we have seen no signs of instinctive tracking, these animals will follow an odor trail on first trial and can learn to follow an olfactory trail or to avoid such a trail. If a maze problem presents such a trail the result is an initial and total accuracy which is greater than normal although the final accuracy, when the trail is in the *cul de sacs*, is less. The learning time is also shortened. We should therefore say that such an olfactory control distinctly favors accuracy.

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[8] Op. cit., p. 232.

[9] Kinaesthetic and organic sensations, etc., p. 91.

How shall we explain this increased accuracy ? Was it a result of real sensory discrimination ? It can be explained, as the results in the black-white maze were explained, as being due to the dominance of some particular stimulus. A path, out upon which an animal first runs in a maze, if not alarming, becomes a familiar place—a home place. There may afterward be other such places in the maze, but this is the first one. He runs out from here, returns, goes a little farther, etc., but always with the possibility of the home return. In Experiment 1, the path was associated with a strong odor trail. Departure from this was to go into the unfamiliar and strange. Thus from the first the animal had more of this stimulus and it became increasingly familiar and increasingly dominant. Dominance, as a term here, may be explained in one way as the power of the familiar. It may have other explanations. Rats are seemingly possessed with an instinctive curiosity or tendency to explore; but fighting against this is an innate tendency to keep in familiar or known situations. The familiar or known situation in Experiment 1, was near the odor trail; in Experiment II, it was away from it. If we accept this view the odor stimulus would be powerful enough to keep an animal in the true path if it arose from this path or to keep it from the blind alleys if it lay there. It would work both ways. It would do so by holding the attention to the true path or by catching the attention and so serving as a warning when the animal strayed from the path. The errors would be lessened in either case.

There were actions, however, which seemed to show that this behavior was more than a mere passive affair. I take it that an instant response to a stimulus, when not instinctive,—a response which can be learned and which can be varied, now positively and now negatively—involves discrimination. There was none of this seen in the black-white maze. There was such behavior here. If this be the case, while the first explanation may be a true and a reasonable one, the increased accuracy here was partly, at least, a result of discriminative ability.

There was an increase of errors in the middle of the learning period in Experiment II, and some slight evidence of the same thing in Experiment I. (See curves Fig. 1 and 2).

The only interpretation 1 can offer is this: It was the result of the changing sensory control. The initial control was dominantly olfactory: but with repeated trials the kinaesthetic experience grew and strengthened and finally began to come into its own. The running became easy and rapid and the accuracy was becoming habitual. Attention, now being released from the control of the movement, was free to be attracted by the olfactory trail in the *cul de sacs* and errors became more frequent. The final elimination may have been, and probably was, a relearning with kinaesthesis more firmly established. But besides accuracy there is also speed to consider.

The conditions of the two experiments give results which differ radically here. As compared with the normal maze, Experiment 1 showed slow initial speed and quick final. Experiment II showed quick initial speed and slow final. Let us first discuss Experiment II.

There is no need to take much time here to discuss the speed in Experiment II. The true path resembled that of the normal maze and the beginning speed was comparable. The slower final speed was a result of the increase of errors. The variable curve seen in Fig. 3 has the same explanation. But let us turn to Experiment 1, where the facts are better seen.

Olfaction has two uses. First it functions as a distance sense. The reaction in this case is always running—toward food, away from danger. The second function is associated with food-taking. Olfaction is so intimately associated with food-taking that, in man, taste and smell are difficult to disassociate. The point which is here to be emphasized is that when the second of these functions is set up in animals in connection with food it inhibits the first. It seems probable that olfaction furnishes animals with a more accurate criterion of distance than it furnishes man and that the nearness of food, with the consequent increased intensity, is the stimulus for the food-taking reaction and the running ceases or slows up. The one response is anticipatory, as Sherrington says,[10] the other consummatory. The one is a somatic reaction, involving the whole body, the other is visceral and confined to certain organs and segments.

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[10] Sherrington, C. S. Integrative action of the nervous system.

If we now attempt to explain the slowness of the reactions of the rats in the maze in Experiment 1, there are several possible interpretations: First, the slowness may be due to the fact that the odor of the food box which served to initiate the reaction is swamped, overpowered, by the nearer, more potent odor of the trail; or, second, that attention is divided between the two and hence we have the characteristic behavior; third, it may be that the pleasurable feeling set up by the odor of the trail is in itself a deterrent and results in loitering; or fourth, it may be that the nearness and strength of this stimulus does initiate the preliminary instinctive food-taking reactions which of themselves end or modify the distance reactions of running.

As one observed the behavior in the initial trial, there did not seem to be any emotional excitement which would suggest the inhibition of running through conflicting motor tendencies and hence the second explanation is discredited. That the trail odor was the predominating one in the first trial seems probable and that it was also pleasurable. The satisfaction of hunger at the end of this trial, however, must, in all succeeding trials, have played a large part and made the original trail a different more intense, more stimulating trail, a somewhat else, *viz.*, a trail which ended with this satisfaction. Yet still there was the loitering and slow movement through all of the early trials which would lead us to think that the fourth supposition may be a reasonable one. Why, then, did this behavior alter in the later trials ? Because of the organization of the whole response into an habitual motor series which only required the odor for the initiation and possible reinforcement of the act. The more rapid final speed, which exceeded the normal, may have been caused by the reinforcement of the kinaesthetic control, now established, by the olfaction of the trail.

Miss Richardson says,[11] "Olfaction may accelerate or retard the learning process; accelerate when the odor is a part of the stimulus connected with the problem—otherwise be disadvantageous." It is easy to conceive that it may have the same effect upon the actual rate of running—that it may result here in a genuine acceleration of speed.

While the main purpose of this work was to establish and to study the effects of an olfactory control in the maze, one of the most interesting features of the results was the proof of a transfer of training. So far as the writer knows there has not been shown before in the animal world, at least in such a graphic way, this change of sensory control from one form to another within a single learning process.

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[11] Richardson, F. R. A study of sensory control in the rat. *Psych. Rev. Mon. Sup.*, **12**, no. 1, p. 68.

THE CHICAGO EXPERIMENTS WITH RACCOONS

L. W. COLE
University of Colorado

At the University of Chicago, three of Professor Carr's graduate students, Dr. W. S. Hunter,[1] and Messrs. F. M. Gregg and C. A. McPheeters[2] have been engaged in repeating experiments similar to mine on raccoons, with results which are most gratifying to me.

Hunter (p. 46 and beyond) found the behavior of the raccoons as different from that of his dogs and rats as I found it different from the behavior of cats. He was compelled, as a result of his experiments, to give up the mere sensori-motor explanation of the behavior of these animals, nor could he attribute it to the association of motor impulses with a whole situation. Motor attitudes could, he thought, account for the behavior of the rats and dogs. It would not serve for an explanation of the reactions of the raccoons. At the close of my experiments, I, too, was compelled to regard those explanations as inadequate. He found that children and raccoons could respond successfully to a stimulus after a much longer delay than could the rats and dogs. He found for the raccoons a maximum delay of twenty-five seconds. The longest delay that I used was at least six seconds, or possibly nine seconds, if we consider only positive reactions of the animals. He compares the behavior of the raccoon favorably with that of a two-and-a-half-year-old child. Moreover, he admits an idea as a "possible cue" used by the raccoons and the children, as against purely motor or sensory cues, used by the other animals tested, though he prefers to attribute the reactions of the raccoons and those of, at least, the youngest child to "imageless thought."

Now that my experiments have been confirmed so fully I must regard them as established. This seems to me to be an item of progress. The psychology of mammals must now cease to be a mere generalization of the psychology of cats. And two of my former students, Professor DeVoss and Miss Rose Ganson, have recently shown what I believe to be an excellent reason why cats may not be expected to behave the same as animals with less defective vision.

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[1] Hunter, Walter. S The delayed reaction in animals and children. *Behavior Monographs*, vol. 1, no. 1, 1913.

[2] Gregg, F. M. and McPheeters, C. A. Behavior of raccoons to a temporal series of stimuli. *Jour. Animal Behavior*, 1913, **3**, 241-259.

We have, then, been driven from the cover of accounting for all mammalian behavior by the sensori-motor hypothesis alone, and psychologists are free at last to try to learn how animals differ in their behavior, instead of denying all differences. This will help enormously, for it may enable us finally to discover a psychology of the higher animals which can explain as well as deny, which can be taken out of the laboratory and yet bear the light of day and the scrutiny of intelligent persons who observe animals. This we have not had. When you meet an observer of horses, who thinks his horse remembers its home, you do not convince him by denying his statements and the evidence he gives, or by calling him "naively anthropomorphic," or by telling him that he did not record the date of the occurrence, or by hurling at him the anathema of "anecdotal psychologist" with opinions "too trivial for serious analysis or notice". A science which can only deny everything and explain nothing is no science and will never receive nor deserve confidence. It certainly was legitimate in 1898 to start by denying the worth of anecdotes of animals for comparative psychology, but only if by denying them we should eventually find a way to explain them, or at least to explain observations of animal behavior which are made almost every day. Experimental animal psychology is now sixteen years old. Consequently it must soon cease to be a generalization of the behavior of cats and take some step which promises eventually to explain animal behavior. Otherwise it must confess bankruptcy and its inferiority to common sense, and remain a sort of science which cannot emerge from the laboratory and which cannot be believed by the psychologist himself the moment he emerges from it.

I am in no hurry for this science to make progress but I should like to see it *take a direction* which promises something. I do not think that a devoted effort to adhere to an objective nomenclature, or to hang the fate of progress on some word, as behavior, or behaviorism, or forever to deny what many observers affirm, is taking a promising direction. It is true that the professors at the University of Pisa saw Galileo drop the weights, and saw them reach the ground at the same moment, and yet refused to believe the evidence of their senses. Animal psychology which merely denies has had an influence in university circles similar to the influence of Aristotle on the professors at Pisa, but it has gained no such influence upon intelligent observers elsewhere. Instead of denying all psychic traits to animals would it not be better to deny our competence to explain more than the merest trifle of animal behavior? I believe that Hunter's confirmation of my results should give a new stimulus to investigators to devise ingenious new experiments suited to find new facts. That avenue seems more hopeful than a denial that there are new facts to be found, and affirming that animal psychology must become a sort of "organic physics."

It is of interest to observe also that while current mammalian psychology cannot come out of the laboratory, common sense observations continually find their way into it. In this paper of Hunter's, for example, one animal is a "stranger" to another and so pays close "attention" to the latter's movements. Preliminary experiments make his animals "acquainted" with the place and apparatus. The raccoons display a directness and "sureness" in their behavior which defies the mathematics of chance. Their "attention" was "distracted" by "yelling at them at the top of my voice" (P. 71). (A procedure likely to make them fierce beyond recall, and which, perhaps, explains the last statement of Dr. Hunter's paper. It is gratifying to learn that this method of distraction was used only infrequently.) Attention and association are everywhere ascribed to the animals and not the association so accurately described by Thorndike, but association pure and undefined. Surely these are greater and more gratuitous assumptions to make than that a horse remembers his stable, even when distant from it, or that a raccoon remembers the box from which it is difficult for him to escape.

I realize that these remarks will expose me to the charge of being as completely deceived as was Herr von Osten, but his is not my position. My view is that "imageless thought," if Hunter's hypothesis is deemed correct, or, at least sporadic, images if my own explanation is accepted as the simpler one, are perhaps so rare in animal experience that the most refined experiments will be required to discover and identify them; experiments beside which Hunter's experiments, and mine, will pale into insignificance, because of their simplicity.

There is still another reason for hoping that the study of animal intelligence may sometime get beyond the stage of dispute and denial. Dispute and denial are poor material to occupy the time of college students. Long ago I had to give up that kind of teaching ,and occupy myself with the more solid information which we possess, of animal sense organs, because dialectic should be taught in philosophy and not in science. Note the extensive work of Kafka[3], the first volume of which has just appeared.

Dr. Hunter's agreement with me does not end with the facts noted above. He is almost persuaded to credit my experiments in putting the animals through the act to be learned, because he has observed the same sort of behavior in rats. At least he admits my apparent credibility relative to my four raccoons. It would be unfair to him, however, not to state his qualifications. On page fourteen he says: "Now with reference to that type of experiment in which the problem learned is that of working latches rather than climbing into boxes, I believe the data presented by Cole are conclusive, *as far as the facts are concerned. Some* raccoons *at least appear* to learn by being "put through." Whether *all* raccoons would do so is, *of course*, *quite* another matter." (Italics mine.)

The reader may reply, "You can surely get but cold comfort from this admission." It gives you the merest semblance or 'appearance' of credibility with regard to your report on your four animals alone. I at least have not charged you with having invented your records." True enough, but the admission means that Hunter's rats, if they have not made a breach, have at least made a weak place in the blank wall of opposition and denial. The latter is definitely given up. How wonderful is the rat at undermining!

The cold comfort comes from the facts that those experiments of mine have not been repeated at Chicago University. I fear, because their raccoons would not permit it (Hunter p. 86). Now should the Chicago laboratory secure a toothless raccoon what may not become of the credibility, temporarily and with qualifications, accorded me ? But what disposition, pray, will then be made of the behavior of Hunter's rats ? With many misgivings, therefore, I await the report of experiments which may even now be in progress.

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[3] Kafka, Gustav. Einführung in die Tierpsychologie. Leipzig, 1914.

In instinctive behavior the Chicago raccoons confirmed my observations rather than those of Davis. Yet I am sure Davis's report is correct, despite the authorities quoted against him, for I saw occasional cases of what he observed regularly. We must not be too cocksure in these matters. Remember that Audubon never saw his pet raccoon wash its food in the water beside it (Davis p. 451).[4] Yet that behavior gives to the raccoon both the name "lotor," and the name "Waschbär".[5]

Interpretations: The reader who is familiar with Dr. Hunter's thesis will recognize the agreements I have mentioned between the behavior of my raccoons and those of the Chicago laboratory. Our interpretations of this behavior are entirely different, of course, except that we were both forced to give up the sensori-motor explanation. Forced from that position, I thought the animal might have memory, or at least a few memory images carried in visual terms, hence a visual image. I still believe that this is the simplest, or as some prefer to say, the most "parsimonious hypothesis." Hunter prefers the assumption of "imageless thought" or "sensory thought" to account for the raccoons behavior, and for that of at least the youngest child. This "imageless thought" must be, at least partly, *visual*, for he says (p. 74), "In the present case there seems to be no room for doubt that the object reacted to was the light." The reader must remember that this light had been turned off for twenty-five seconds before the animal was permitted to react to it, in the maximal delays with raccoons, hence the "representative functions," next mentioned. For he continues thus: "Now if a representative function were involved in the behavior of the reagents, *as seems to have been the case with the raccoons and children*, it must in part at least, have been *representative of the lighted box*, because all else—including the three possibilities of movement—was constant from trial to trial, whereas a selective response must needs have an alternating cue". (Italics mine).

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[4] Davis, H. B. The raccoon: a study in animal intelligence. *Amer. Jour. of Psychology*, 1907, **18**,

[5] I have to thank Mrs. R. M. Yerkes for calling my attention to this splendidly appropriate German name for the raccoon, and its superiority to the American name, whose source is not certain.

I know of no way in which light can stimulate these animals except *visually*. And when the animals were permitted to react, it was by means of a function representative (at least partly) of the lighted box. One would think that the simplest escape from this dilemma would be by means of a visual image. But no, it is visual in source or cause, yet imageless in content. We have often been led to believe that sensation gives a rather fundamental content to thought. Perhaps we may now teach that Helen Keller, for example, has both the *content* of visual experience as well as a knowledge of its relations. Loeb[6] has recently given evidence to show that a retinal image produces a brain image, which corresponds with the former point for point. "Diese Tatsachen enthalten aber, wie mir scheint, auch den Nachweiss, dass in Gehirn ein Bild der gesehenen Gegenstaende entsehen muss" (p. 1016). By Hunter's hypothesis all of this image forming apparatus is rather useless, for no mental image arises in the raccoon, nor perhaps in the youngest child, under the conditions of the experiment.

Doubtless it will occur to the reader of Hunter's paper that this explanation of the raccoons' behavior, by means of imageless thought, was in no way suggested to him by his experiments and seems to be a rather foreign addition to his thesis, forced upon him by the milieu or suggested by current discussions of the topic in human psychology. In order to use the concept to account for the results of his experiments he must make the claim (p. 77) that imageless thought is genetically prior to thoughts with images, and he must dismiss the opposite teaching as having "no factual basis" but seeming to be "the result of prejudice or of temperamental leaning." Then the point of origin of imageless thought is placed "at least as low as the raccoon" (p. 77). All this seems a trifle complex to me but the actual advance made is, now that the old explanation has been given up, that the reader may choose what hypothesis he will under the law of parsimony.

Doubtless psychologists will be more interested in Hunter's immediate explanation than in his final one, which I have already outlined. When the conditions of his experiment demanded that the animals go to an electric bulb, whose light had been extinguished some seconds before, in order to execute a successful reaction, the rats and the dogs oriented toward the light, either with the whole body, or at least faced in its direction. They kept this orientation during the period of delay in so many of their correct responses that this "motor attitude" evidently served to bridge the time gap between the disappearance of the light and the release of the animal. Consequently their "motor-attitude" accounts for the success of the rats and dogs. The raccoons and the children did not even face the light in so great a proportion of their successful responses that the "motor-attitudes" hypothesis breaks down completely, as an explanation of their behavior.

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[6] Loeb, J. Die Bedeutung der Anpassung der Fische an den Untergrund fuer die Auffassung des Mechanismus des Sehens. *Zeit. f. Physiol.*, 1911, **25**, 1015-1017.

As a result of this outcome of the experiments, Hunter (p. 80) decides that, "Some intra-organic (non-orientation) factor not visible to the experimenter must be assumed in order to explain a significant number of the correct reactions of the raccoons and all of the successful reactions of the children. These cues fulfilled an *ideational* function." (Italics mine.) And again (p. 72), "As we have indicated, such a mechanism would apply only to the non-orientation cues used by the raccoons and children. The type of function here involved is *ideational* in character. By applying the term "ideas" to these cues, I mean that they are similar to the memory idea of human experience so far as *function* and *mechanism* are concerned. They are the residual effects of sensory stimuli which are retained and which may be subsequently reexcited. The revival, moreover, is selective and adaptive to the solution of a definite problem, and when aroused, they function successfully as a necessary substitute for a definite component of the objective stimulus aspect of the problem." He has already said that the effective component of the stimulus was the *light*. Unless he denies, then, a visual content to this "factor," it is a visual, imageless thought. But since he does deny it a representative content, though it has a representative function, he terms it "sensory thought," though the stimulus has been absent twenty-five seconds in the longest delays of the raccoons. This "sensory thought" then becomes the imageless thought of current discussion, by the genetic reversal of current opinion on that subject that I have mentioned above.

It is interesting to observe how very "similar to the memory idea" is this "intra-organic factor." It is a residual effect of a sensory stimulus. It may be retained and revived, is selective, etc. Elsewhere (p. 69), he describes this factor as "Some *unknown* intra-organic cue non-observable by the experimenter. Our data prove conclusively that some such cue was utilized by the raccoons and the children, ... the nature of such a factor must necessarily be defined at present in *negative* terms." When this statement was written it evidently had not occurred to Hunter to place this negative thing in the positive category of imageless thought. His experiments were completely described before reaching this point. Hence, it seems to me that imageless thought was an afterthought, as an explanation.

On the second page of the paper we find this significant statement. "In the interpretative discussion at the close of the present monograph, we shall be confronted with the possibility that images or ideas *may* have guided the reactions of the subjects. In discussion, we shall assume that there is no necessity that psychology postulate such a representative factor save where successful reactions occur in the *absence* of the stimulus (object) or movement represented." So images *may have been present.* Yet throughout his references to my paper Hunter complains that I did not reach a *proof* of the presence of images. When his experiments were completed, he seems to be in much the same position. Just how an experimenter can give *proof* that animals remember or think, even in imageless thought, I am quite unable to guess. I thought that my animals gave *evidence* of possessing visual memory. Hunter's experiments strengthen this opinion of mine very much.

Like Brehm and all subsequent observers of the raccoon, Hunter has noted the fly-catching activities of this animal. He consequently accords to the "Waschbar" the possession of acute vision. In this he agrees with my report.[7]

Errors: On page eighteen, in re-describing some of my experiments, Hunter says, "a block with a *steeple* was placed in a hole," etc. With absolute confidence I must assure the psychological public that I used no "steeple" in my apparatus.

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[7] Cole, Lawrence W. Observations of the senses and instincts of the raccoon. *Jour. of Animal Behavior,* 1912, **2,** 302.

I have, very rarely, *heard* the word "steeple" used for "staple," but never before have I *seen* it so used in a scientific monograph.

Again, I am regarded as having been "misleading" (p. 86) in my statement that "the year-old raccoons apparently are not quite full grown," for Dr. Hornaday and Mr. DeVry say "that raccoons reach maturity at three years of age." But do Dr. Hornaday and Mr. DeVry mean, therefore, that the raccoon accomplishes but one third of his growth each year, as Hunter seems to interpret them ? I cannot believe it. I kept my animals three years and I wish now to re-affirm the statement above. They grew but little after the first year. Work and confinement may have stunted them, though they were fed each day to satiety. In parks I have now seen many raccoons of about the same size which mine attained. They had been in confinement for a long time so they must have been full grown. I have also seen a number of much larger specimens.

Criticisms of my Work: The introduction to Dr. Hunter's thesis takes the form of a fearful arraignment of both my experiments and my arguments. To use his own phrase, most of the latter "can be dismissed summarily" (p. 16). They are in turn dismissed summarily in favor of the sensori-motor explanation, so his theory of raccoon behavior at the beginning of his paper differs entirely from that at its close. I suppose that I ought to make some reply to these criticisms, but I shall be as brief as possible and at that I shall select only the most important ones. It seems better to omit any answer at all to such remarks as, "To some it may seem too *trivial* either for serious analysis or notice" (p. 10), a criticism which I seem to share with Lloyd Morgan and others, save that I have *persisted* in their trivialities.

Criticism 1. "Hence assuming the facts that Thorndike and Cole assume to be unquestionable, it need only follow that the raccoon exhibits more complex sensori-motor behavior than the dog and the cat, and not that it shows a new type of behavior, i. e., a type of behavior involving the functional presence of a representative factor." (P. 15.)

Reply. Yet he later found just such a factor functionally present in raccoons.

Criticism 2. "To argue that this means *image* of apple is certainly naive at least. Could the raccoon not sense the apple when his nose was within a foot of it ?" (P. 18.)

Reply. Hardly probable, since the floor between him and the piece was carefully rubbed with another piece of the same apple, and his forepaws were still moist with the pieces of apple he had already eaten. But note Hunter's argument (p. 27) that smell was eliminated in his experiments because the rat was given only a bite, "so almost no food fell on the floor." Food was used with the raccoons in the same way.

Criticism 3. Varying means to the same end. My data under this head are just as inconclusive as that presented above. (P. 17.)

Reply. Curious then that Professor James thought this "the mark and criterion of the presence of mentality in a phenomenon." "We all use this test," says James, "to discriminate between an intelligent and a mechanical performance."

Criticism 4 "The criticisms on Cole's entire work reduce to these: (1) The facts are either inconclusive or irrelevant. And (2) there is no evidence of adequate controls." (P. 20.)

Reply (a) Why then are the same facts, namely, responses to an absent stimulus, so satisfyingly conclusive of imageless thought ? This recalls the remark of Hodgson, "What you know least about, assert to be the explanation of everything else." (I quote from memory.) (b) "No controls." This is the repeated cry in these papers. It seems probable from the statements of the papers that their authors did not read my account and that they misunderstood the few pages they did read. I shall show this in detail in showing that Gregg and McPheeters (and their experiment was planned by Hunter) have entirely misunderstood what I did.

In concluding the discussion of Hunter's report alone the points of similarity between his experiments and mine may be enumerated. He extinguished lamps which were used as stimuli, while I put a series of objects in view of the animal, then out of view again, and he must discriminate, under these conditions, between absent stimuli. Hunter secured delay by caging the animal, while I secured it by not feeding the animal until every member of the series had been put in view and (except the last member) out of view again. Sometimes six objects were used by me (i. e., each of three cards was shown twice). Hunter found it inapplicable to use the third light in many cases.

The second paper, that of Gregg and McPheeters, had no other object than "to demonstrate the inadequacy of Cole's experiment." (P. 258.)

They reconstructed my "card-displayer," except that the levers were not screened from the view or touch of the animal and a system of strings and pulleys was added which the animal could also see.

Then they gave the two raccoons *two days training on the levers alone without any cards attached to them.* (P. 245.) One of the two animals, Jack, failed utterly to discriminate. "Further training *might* have developed discriminative reactions in his case but time did not permit a continuance of the tests." (P. 246.) Jill discriminated between the two series on some basis, but Jill also "soon acquired the habit of standing close to the levers and touching her nose to them as they appeared." (P. 247.) Here, the reader will doubtless say that *all* analogy with my experiment ends. I should agree to this so far as the method and apparatus are concerned but it seems easier to change those than to change the nature of the raccoon, for there is a startling agreement between the behavior of their one successful animal and my four.

Let us find this agreement. In the training series Gregg and McPheeters kept two constant factors. (1) A "normal" order of lever positions used, according to their respective distances from the animal. (2) They always presented the levers in series of three. Jill reacted to the order of lever positions chiefly, perhaps (p. 249), but she responded partly to the threes, for they say (p. 252), "Positive reactions of food getting may be stimulated successfully by any of the following groups, 1-2-3, 1-3-3, 1-2-2, 2-2-2, or 1-1-1. Likewise, inhibition, or negative responses may be stimulated by either group 3-3-3, or 2-2-2. The nature of the stimulus is relative to the character of the group with which it is *alternated*." One cannot help asking, why continue to alternate by threes only, unless they meant to teach the animal to respond to alternate threes ? Why not alternate by sixes as I did ? This was one of my "controls," which they have overlooked.

Jill reacted to the two constant factors. In my experiments only the color (and brightness) of the cards was kept constant. My four animals responded to that. In both the Chicago experiments and my own the raccoons responded *to the constant factor.* What more could they do, pray ? This seems to me to be an excellent example of the method of agreement.

But this was the behavior of Jill, the single raccoon which succeeded in discriminating in their experiments. One would suppose that no very weighty conclusions would be drawn from the behavior of the animal which failed. But he is said to have responded to the *sounds* of the levers. Their "usual sound." (P. 248.) (Why not make the levers noiseless ?). This animal then responded to sound, perhaps partly to lever order and, I have no doubt, to any other element of the situation which was left constant, and which also enabled him to get food. "He seemed to watch the peep hole, although possibly he was merely listening for some sound upon which to base his reactions" (P. 246), so they set a metronome going to drown the noises made by movements of the experimenter!

I confess I can see nothing in these experiments except a rather determined effort to divert the animal's attention from the cards and to get him to respond to the levers. The following items seem to show this:

1. Two days preliminary training on levers alone.
2. The board screen was reduced to "about five inches" in width (p. 244), thus showing apparently two thirds of the length of the levers, if Figures 2 and 4 (pp. 243 and 247) correctly represent the apparatus.
3. Putting the cards above the raccoon's line of vision, if Figure 4 is correct.
4. Converting my visual experiment into a tactual one by letting the raccoon touch the levers.
5. Adding the cue of noises in operating the levers, as well as noises due to the experimenter's movements.
6. Each of the three strings attached to the levers (Figure 4, p. 247) must have changed from slack to taut *before* the lever appeared, thus further directing the animal's attention to the levers' positions. I am unable to find "controls" against the animals having reacted to the strings.
7. Feeding the raccoon for having reacted to the levers.
8. The colored cards were much smaller than mine.
9. Finally only one of their animals succeeded in discriminating as compared with four of mine.

"The essentials of Cole's apparatus and method were duplicated in our experiment." (P. 244.) Truly, with all these carefully arranged differences, I am quite unable to find that the "essentials" of the experiment were even *similar* to mine, but the reader may judge for himself. Any one who is familiar with my paper will remember that only a small part of the upper portion of the lever projected above the screen board. To be specific, my notes of Dec. 6, 1905, state that the lever "when upright" extended "one inch above the upper edge of the front piece."

"*Controls*" By the charge that I did not employ "adequate controls" is meant chiefly that I did not guard against discrimination by position of cards and levers nor against discrimination by cues given by the experimenter. Let me call attention to two items which my critics have overlooked relative to the first precautions, and quote from notes of the experiments. On page 228, I say, "During one test red would be on the forward lever, one inch in front of the other, during the next test on the rear lever. The animal could not, therefore, react to the position of the cards." I did not re-state this precaution in the portion of my account on which the Chicago laboratory based its experiments, but one presumes that a critic reads completely the paper he criticises. To show that this precaution was kept up during the three-color work I will quote my "daily plan" for one animal for three consecutive days.

"April 23. Jack. Same as preceding. Blue middle, orange back, white front."

"April 24. Jack. Three colors. Orange front, white middle, blue back."

"Jack. April 25. Three colors. Blue front, orange middle, white back."

It is evident that each card occupied every possible position in each three consecutive tests, and that no card occupied the same position for any two tests. Does this look as if I took no precautions against the animals reacting to the positions of the cards? I find no such precautions as this, to leave only the colors constant, in the work of Gregg and McPheeters, so it seems that the animal was fed for depending on another cue.

But the uninformed reader may ask, "But what of *lever* position?" At the beginning of each days work the levers were "strung" on their supporting pivot in any order in which they were picked up. We did not remove them from the room in which the raccoons were kept and we generally found them scattered about the room. The levers were all alike so far as we could detect, until having split one, we replaced it with one having a "new" appearance. This should have brought a new type of result if the animals were responding to the appearance of the levers.

It is true that this change of card position is mentioned briefly at the outset of the experiments instead of within the portion on which the Chicago experiments were based. But was no further precaution taken which was described on the final pages of report? On page 259, Table 11, I record that for two hundred trials the threes were "shown twice." Since this has been unnoticed or misunderstood let me explain it. It means that I would show red, red, red, red, red, red, and the animal must stay down through it all. Then came white, orange, red, and only then would the animal climb up on the high step to be fed. Thus nine movements were made, and *all the levers were used.* Then followed white, orange, red, and the animal reacted positively and was fed. Thus he could hardly have been responding to alternate threes, or to lever position. Note also that there was an abrupt transition from showing the cards by threes to showing them by sixes. Yet the animal gradually learned to discriminate in this complicated experiment in which all factors were different, except the colors of the absent cards. I describe this showing the cards by sixes at the bottom of page 258, and refer to it as "while you raise three or even *six* colors, again on page 261." Perhaps this detailed account of the precautions taken to guard against discrimination by threes, and against discrimination by position will serve to convince the reader that the experiments were not so careless or hasty as my critics have supposed.

But it is further assumed that I mixed the experiments in which the experimenter manipulated the levers with those in which the animal was permitted to claw at them. The two types of experiment were separated by months. My paper states (p. 233) that no tendency to claw at the levers appeared for *six weeks* of the first type of experiment. After it did appear, clawing at the levers was not permitted until we had learned what we could by the experimenter's operating them. Nor did the raccoons attempt to claw at the levers, if the experimenter manipulated them rapidly. In fact we developed the habit by moving the levers slowly. This confusion on the part of Hunter, Gregg and McPheeters appears to be due to my giving a logical, instead of a chronological account of my experiments.

The behavior of my raccoons was not, therefore due to touch. Consequently Hunter's experiments with lights is more similar to mine than that of Gregg and McPheeters, whose "card-displayer" had some points in common with mine.

As to cues from the experimenter, I always extended my hand as if to feed the animal, at the negative as well as at the positive series. My notes contain many instances, at first, of this response to the hand. These were of course counted against the animal, and finally he ceased to be influenced by the movement. Different experimenters operated the levers and, in one case, it was found that the animals were responding to unconscious movements of the operator. This is mentioned in describing the vision of the raccoon.[8] This experience shows that if the mere presence of the experimenter, or his breathing, had been the cue to which the animals were responding the raccoons would have made far better records than they did, and the work of months would have been reduced to days. I should still prefer to have the experimenter present rather than to use the system of strings, which caused the noise, the peep hole, the opening for food, and to permit the noise of the experimenter's movements, which had to be overcome by a metronome, all of which were used by my critics.

Conclusions: It is noticeable that, so far as Gregg and McPheeters draw a conclusion, they ascribe the raccoon's behavior to "motor attitudes," "sensory attitudes" and, if images were present in our animal, they must have been kinaesthetic, i.e., imaginal attitudes." (P. 258.) Thus they give the explanation of the raccoon's behavior which Hunter found was entirely inadequate to account for it, but which, he believes, does account for the behavior of the dogs and rats. Perhaps, at the time their experiments were made, Hunter's results were still incomplete and it was assumed that the raccoon's behavior would be found in nowise different from that of the dogs and rats. At any rate, we now have three different hypotheses to account for the behavior of raccoons.

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[8] Cole, Lawrence W. Observations of the senses and instincts of the raccoon. *Jour. of Animal Behavior*, 1912, **2**, 302.

1. Attitudes, motor, sensory or imaginal. Gregg and McPheeters.
2. Not attitudes, but imageless thought. Hunter.
3. Visual memory, at least sporadic. Cole.

Truly, "Homines perfacile credunt id quod volunt."

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THE WHITE RAT AND THE MAZE PROBLEM:

III. THE INTRODUCTION OF A TACTUAL CONTROL

STELLA B. VINCENT
Chicago Normal College

In two papers, appearing in preceding numbers of this Journal, I have attempted to show that vision and olfaction can be introduced as controls into the maze problem and to demonstrate some of the effects of such an introduction upon the learning process of the white rat. In this article I wish to review, briefly, in the light of the previous discussions, some work on the maze problem where the conditions for tactual and cutaneous control were emphasized and to add some experimentation not previously reported. For the full details of the earlier work see my monograph, "The Function of the Vibrissae in the Behavior of the White Rat."[1]

While this paper, the third of a series, attempts to show how tactual elements enter into and modify the maze reactions, it must be understood that the sensory experience is always a complex. Yerkes has sounded the warning clearly when he says: "An animal responds to a situation, not to any one independent and isolated stimulus. Every situation, to be sure, may be analyzed into its component simple stimuli, but the influence of each is conditioned by the situation."[2] The difficulty of isolating the tactual element is the chief reason why there has been so little work done with it in studies of labyrinth learning The experimentation which has been undertaken up to this time has consisted mainly in moving the labyrinth to a different base, covering the floor path with different substances, interposing hurdles, and the use of anesthetics on the feet of the animals.

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[1] Vincent, S. B. The Function of the Vibrissae in the Behavior of the White Rat. *Behavior Mon.*, vol. 1, no. 5.

[2] Yerkes, R. M. Relations of Stimuli in the Frog. *Harvard Studies*, vol. 2, p. 546.

Opinions as to the value of the sense in such problems have been based upon observation and voiced in general statements like this: "The longer one observes the behavior of the dancing mouse the more he comes to believe in the importance of touch and motor tendencies."[3] Or the assumption was perhaps a specific one and yet unsupported by any evidence, as: "Tactual-motor sensations furnish the essential data for the recognition and discrimination involved in forming the special associations at critical points."[4] One investigator has made apparently contradictory statements, as: "The indications point to the fact that the rat in no way uses his cutaneous sensations as a basis for 'sensing' the correct turns in the maze as distinguished from the incorrect."[5] In this case the feet of the animal were anaesthetized with ethyl chloride. Reporting some experiments with blind animals he said: "Runs squarely down the middle of the galleries, makes his turns into the various entries as boldly and with as much sureness as do the normal rats. The vibrissae undoubtedly play a large part (though not an indispensable one) in the early reactions of these rats to the maze."[6] Of normal animals he remarks: "In all probability the rat does not discriminate his turns by means of any data contributed by the vibrissae." "Vibrissae undoubtedly warn him of the presence of solid objects. . . . The function of the vibrissae to some extent at least may be dispensed with once the path is learned."[7] These seeming contradictions, however, are due to the confusion in the report of those activities involved in the formation of the habit and those essential to its control when established. The conclusions are those drawn from one type of maze and one form of motor habit and while possibly valid in this particular problem cannot be carried over to all such co-ordinations.

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[3] Ibid, Dancing Mouse, p. 178.

[4] Small, W. S. Mental Processes of the Rat. *Amer. Jour. Psy.*, vol. 12, p. 237.

[5] Watson, J. B. Kinaesthetic and Organic Sensations. *Psy. Rev. Mon. Sup.*, vol. 8, no. 2, p. 78.

[6] Ibid, p. 58.

[7] Ibid, p. 69.

Miss Richardson makes some definite statements though not in connection with labyrinth problems: " Slight contact (with plane) seemed to give her immediate orientation."[8] " The basis seemed to be that afforded by touch. Contact with the plane was doubtless evidence of its presence." . . . " It was only when they came in contact with the plane that some sensory impulse connected with its fall set off the old association and they would dash to the door of the box."[9] " There was no indication that any of the rats located the door by means of vision for each rat passed the door while 'searching' for it without reaching to it. Yet when the door was touched there followed the examination of the latch and the requisite movements to open the door." . . . " Locating the door as before probably with the snout."[10] " The normal rats like the blind rats seemed to discover the latch by contact."[11]

A layman would scarcely question the importance of the tactual experience in the life of animals, yet in experimental work its function had been called in question even in such problems as Miss Richardson mentions and kinaesthesis had barred all rival contestants in labyrinth learning. It was in order to test the control in the maze that this work was undertaken.

DESCRIPTION OF MAZE

The method used in testing this tactual control was not quite the same as that employed in the work with vision and olfaction. In those experiments the stimulating values of the true path and the blind alleys were made to differ in as pronounced a manner as possible. In this case there was no attempt made either to accentuate the contact values of the floor or walls of the maze or to offer contrasting standards in the true path and the false. Another maze was built on a new plan where the conditions, it was hoped, were such that not only could the tactual functioning of feet and vibrissae be seen but also that such functioning would be a necessary part of the learning process. (Figure 1.)

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[8] Richardson, Florence. A Study of Sensory Control in the Rat. *Psy. Rev. Mon. Sup.*, vol. 12, no. 1, p. 39.

[9] Ibid, p. 40.

[10] Ibid, p. 55.

[11] Ibid, p. 56.

The runways to this maze had sides which could be detached. When this was done there was left a maze pattern of open, elevated paths but these paths had sufficient space between them so that the animals did not try to jump from one to the other. It was found that on this open maze, where the whole pattern was exposed, the visual control was not sufficient to prevent there being just as real a problem as was seen in mazes with enclosed sides. The situation forced the use of the feet and the vibrissae in a way that the other mazes did not and this fact accounts for the title at the head of this paper. Other sensory elements contributed to the learning, without doubt, but the tactual-cutaneous factors were the prominent ones and the ones which we wished to throw into relief. As it is desired to compare the results obtained in this work with those secured where vision and olfaction were emphasized in the Hampton Court maze, let us compare the two labyrinths.

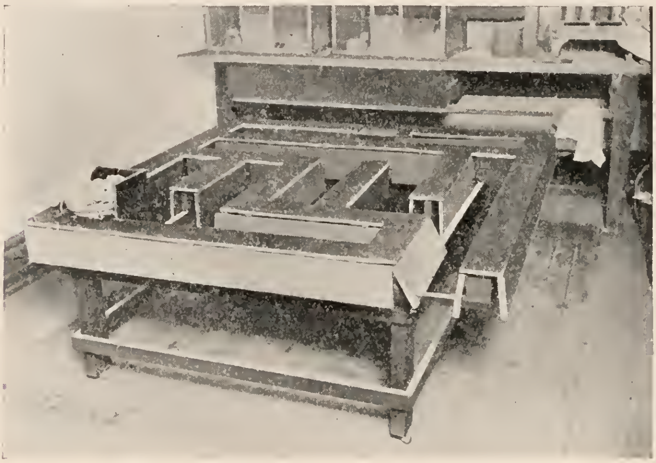

Fig. 1—The maze as used with sides down

COMPARISON OF HAMPTON COURT AND X MAZES

The length of the true path in the Hampton Court maze is 40 feet, in this 17 feet. There is one more blind alley in the H.C. maze than in this. The *cul de sacs* have a total length of 30 feet in the one and 9 feet in the other. The paths, both true and false, of the H.C. maze are more complex in nature. The results obtained from the H.C. maze and those given by the smaller maze, which we will call the X maze, when the sides are on are very similar. In table 1 they are given in tabular form together with the dimensions of each maze.

To make these results comparable it is necessary to multiply the errors of the X maze by 7/6, since the H.C. maze has 7 errors while the X maze has only 6. The time taken to run the maze should be directly proportional to the length of the path. In the first trial in any maze the *cul de sacs* are explored rather thoroughly; therefore the time of the first trial in the X maze should be multiplied by 40/17 x 30/9, the ratios between the lengths of the true paths and the *cul de sacs* in the two mazes. In the final trials, however, the errors are cut out and to get the comparative speed we multiply the figures for the X maze by 40/17 to correct the speed for the true path. By comparing the corrected results of the X maze with those of the H.C. maze we can see that the statement of similarity is substantiated.

The X maze took an average of four more trials to learn than the H.C. maze. The slower learning time for the X maze is doubtless a result of the character of the *cul de sacs*. There are three pairs of blind alleys in this maze. One and three are exactly of the same length and character and so are two and six and likewise four and five. The two latter pairs differ only four inches in length while after the turns the distances in 1, 2, 3 and 5 are identical. (See figure 1.) The distances on the true path between the turns are also comparable. If, after the habit is formed, the running under these conditions is carried on largely in kinaesthetic terms, as we believe, then differences between the kinaesthetic elements in the series should favor such an accomplishment. Such differences in kinaesthetic elements are differences in complexity, differences in the distances between the turns as well as in the direction of the turns, and differences in the lengths of the *cul de sacs*, etc. Too great a similarity between such kinaesthetic units would hinder the learning. The plan of the H.C. maze, according to this conception, is more favorable for learning and hence the slower learning time of the X maze. The corrected figures for the X maze show a greater average number of errors in the first trial and in the last five trials but looking at the average number of errors for the first five trials and the total errors per animal we see that the balance is in favor of X maze.

Thus the error balance in the figures of the two mazes now leans to one side and now to the other. These differences, also, probably spring from the form and character of the *cul de sacs.*

The lower final speed in the X maze is caused by one slow animal. If we take the time for all of the runs in which there were no errors in both series and from these records compute the speed per foot for each maze we find it to be exactly the same, 2.5 feet per second. This is not the final speed, however.

The object here is not to go over these details item by item but merely to show that, in general, these mazes are alike in type and the reactions made in them are therefore approximate.

COMPARISON OF EXPERIMENTS ON X AND Y MAZES

We will now turn to a consideration of the experimentation on the X maze, where the sides to the runways were on, and the same maze, which we will call the Y maze, the open maze, where the runways had no sides.

The behavior in the X maze needs no description but that in the Y maze showed essential differences. When the sides were taken from the runways and the rats put on the maze they showed a marked tendency to follow the edges of the paths. They did this either by turning their vibrissae down against the sides or by curling their toes over the edges of the board. That this was a real control was shown by using rats whose vibrissae had been cut on one or both sides of the head, by using blind rats with and without vibrissae and rats in which the branch of the fifth nerve which innervates the upper lip and snout had been cut. The learning in all of these cases was made more difficult except in one instance. In this case the vibrissae were cut on one side only. As a result, the animals were forced to keep to one side of the maze and by following this side they made their way around the labyrinth almost immediately. It is impossible here to go into all of the evidence and readers are referred to the original monograph.[12] The work conclusively showed that the tactual-cutaneous experience had a vital part in the solution of the problem. In the end the rats ran this maze with as much boldness and confidence as the other, with heads up, almost leaping corners, etc. The one exception was the group of blind rats without vibrissae.

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[12] Op. cit.

Let us compare the results of the two mazes as to accuracy and speed. We find that the time of learning was the same but in the Y maze the errors were less by one-half in the first trial and one-third in the first five trials, and the total number of errors was decreased about one-third although the final accuracy of the two mazes was practically the same. The beginning time was shorter because of the fewer errors but the average time of the first five trials was about the same in both. The final speed in the Y maze was slightly better. The most noticeable difference, then, was the decrease in errors. The open maze, from the beginning, favored accuracy and it should be noted that this accuracy was not attained at the expense of speed.

In a maze, where the paths are enclosed by restraining walls, there is little need of fine bodily adjustments. The turns in the H.C. maze and in this maze are always 90 degrees but the place of the turn in the H.C. maze is always marked by some corner or projecting wall against which the body of the rat brushes or his vibrissae drag as he runs. A railway engineer does not have to keep his train on a straight course by the fraction of an inch, he has only to develop speed, his track is laid for him. The analogy is not perfect but in the enclosed maze the rat is comparatively "safe." He does not have to control, as on the open maze, the finer postural and positional adjustments and as a result of this looseness of running he makes more errors. On the open maze the control of these finer adjustments is necessary in order to avoid slips and falls and hence there is greater initial and final accuracy.

The nose, feet and vibrissae were constantly used at the different places of turning. The direction of the turn seemed a much easier thing to conquer than the exact place. The operated animals were at a great disadvantage. Vision aided these finer adjustments but the nose and feet and vibrissae seemed to be of greater help to the rat than sight. However, either sight or the touch of nose or vibrissae seemed to be a vital necessity to the learning. The animals could not well dispense with both in such a problem as was here presented.

THE X MAZE RE-LEARNED AS THE Y MAZE

That the habits set up in the two mazes were inherently of different type was shown by the following experiment: After the group of animals whose records are given for the X maze in table 1 had learned the maze the sides were removed and the rats were tried again. Kinaesthesis had apparently been firmly established during the first experiment and while some disturbance was to be expected, it was thought that it might affect the runs of but one day. The outcome shows the danger of supposing anything about animals. These rats had to relearn the maze almost as if it were a new problem. The old habits did not meet the situation. The animals went out upon the maze with flattened, crawling bodies; they clung to the edges with their toes, they followed these edges with their vibrissae; they used apparently every tactual-cutaneous help possible. While the fewer initial and total errors seem rather good evidence that something was carried over from one maze to the other, the fact that it took over eleven trials on an average for the relearning, as well as the evidence of the observed behavior, indicates that the habit had to be re-established through new sensory aids. A summary of the numerical data may be seen in the last column of table 1.

The maze pattern was the same. The kinaesthetic series was the same: the distances, turns, all that goes to form what Professor Watson calls a kinaesthetic element, but the other sensory elements, always present in the kinaesthetic complex, light, possibly odor and sound but chiefly touch had greatly changed. Always, as the rat ran in the X maze, his sides and vibrissae brushed the walls, the projecting partitions and the angles of the box. All at once this part of the sensory experience was gone. It could be and it was replaced but with a tactual experience of another sort requiring very different adjustments. In addition there was the necessity for the finer adjustments previously mentioned. Thus the problem became a new one. The position which I desire to maintain here and upon which I desire to lay emphasis is that, while in a fully formed habit kinaesthesis probably predominates as a control, the sensory experience is never purely kinaesthesis but always a complex and the finer are the adjustments which need to be made the more necessary the associated sense qualities of vision and touch become.

TABLE 1

Comparison of Mazes—Dimensions, Records

	Hampton Court Maze	X Maze	Y Maze	X Maze Corrected	X Maze re-learned as Y Maze
Total length of true path...	49 ft.	17 ft.			
Length of *cul de sacs*.......	30 ft.	9 ft.			
Number of *cul de sacs*......	17	8			
Number of turns, true path.	19	7			
Time of learning...........	12.1 ± 3.6 trial	16.5 ± 3.7 trial	16.5 ± 6.6 trial		11.8 ± 9.2 trial
Average errors first trial....	14.7 ± 7.7	18 ± 8.8	9.5 ± 4.05	(x 7/6) 21	4.6 ± 1.3
Average errors first five trials..................	48.7 ±17.7	30 ±12.8	21 ± 5.2	" 35	11.4 ± 3.3
Average errors last five trials..................	.1 ± .14	.18 ± 2.7	.09 ± .11	" .42	0.
Total average errors per animal..................	66.6 ±16	51 ±23	37.3 ± 5.1	" 59	22 ± 7.6
Time first trial............		7.2 ± 3.8	5.1 ± 2.5		2.9 ± 1.2
Average time first five trials.	16.3 ± 6.7 min.	1.8 ± 2.7 min.	1.9 ± .7 min.	(x 40/17 x 30/9) 14.1 min.	1.19 ± .4 min.
Speed last five trials........	.31 ± .05 min.	.18 ± .06 min.	.15 ± .03 min.	(40/17) .42 min.	.1 ± .01 min.
Average speed in trials without error................	2.5 ft. per sec.	2.5 ft. per sec.	1.8 ft. per sec.		1.8 ft. per sec.
Total surplus time.........	93.9 min.	15.3 ± 6 min.	13.9 ± 4.3 min.		8.93 ± 2.38 min.

CONCLUSIONS

The conclusions from this study are that, given conditions which favor or necessitate the use of vibrissae or the tactual use of nose or feet, the maze habit is not more quickly established but that during the setting up of the habit fewer errors are made and because of this the time per trial is lessened and time is gained. The conclusion is also drawn that these conditions make, within the limits of the experiments, for greater final speed as well as for greater final accuracy.

A STUDY OF THE BEHAVIOR OF THE PIG *SUS SCROFA* BY THE MULTIPLE CHOICE METHOD

ROBERT M. YERKES AND CHARLES A. COBURN
The Harvard Psychological Laboratory and the Franklin Field-Station

INTRODUCTION

The multiple choice method of studying ideational and allied forms of behavior was first briefly described in a lecture on the study of human behavior delivered at Cold Spring Harbor in 1913.[1] It has recently been more fully described in a paper which presents the results of its application in the study of the crow.[2] We shall, in the present report, assume knowledge of the previous descriptions and state only the essential features of the method and its adaptation to the organism observed.

It was devised in the Psychopathic Hospital, Boston, as a means of obtaining comparable records of the ideational behavior of mentally deficient and deranged individuals. But it was also hoped that it might prove widely serviceable as a comparative method for the study of various types of organism.

In many of its essential features, the Yerkes multiple choice method is similar to the Hamilton quadruple choice method,[3] but whereas in the latter four reaction-mechanisms are employed and only problems which, strictly speaking, are insoluble are presented to the subject, the present method involves the use of a variable number of reaction-mechanisms and the presentation of soluble problems of a wide range of difficultness.

The experimenter seeks, in using the multiple choice method, to present to his subject, no matter what its type, age, or condition, a problem which may be solved by the perception of a certain constant relation or group of relations within the reaction-mechanisms. For example, the mechanism to be operated may, in the case of one problem, be the middle one of the group, and the total number of mechanisms presented may vary from three to nine. Only by perceiving and appropriately responding to the relation which the experimenter designates as middleness, can the subject solve the problem.

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[1] Yerkes, Robert M. The study of human behavior. *Science*, 1914, **39**, pp. 625-633.

[2] Coburn, Charles A. and Yerkes, Robert M. A study of the behavior of the crow *Corvus Americanus* Aud. by the multiple choice method. *Journal of Animal Behavior*, 1915, **5**, pp. 75-114.

[3] Hamilton, G. V. A study of trial and error reactions in mammals. *Journal of Animal Behavior*, 1911, **1**, pp. 33-66.

It is necessary only, in the presentation of a varied series of multiple choice problems to a given subject, for the experimenter to devise a type of reaction-mechanism which is appropriate to the action-system of the organism to be observed. We have thus far made use of a simple keyboard for human subjects, while for crows, ring-doves, and rats, we have employed a series of similar boxes, each with entrance and exit doors which can be operated at a distance by the experimenter. The form of device which has proved suitable for the study of pigs will be described in this report.

It has proved very easy to develop suitable mechanisms and we have every reason to suppose that this new method has great advantages over most others for the comparative study of behavior in that essentially the same problems may be presented to extremely different types of subject.

The method has been employed in experiments with normal and defective children, normal and insane adults, pigs, rats, crows, and ring-doves.[4] To all of these subjects, four problems have been presented. They may be described briefly, by definition of the correct reaction-mechanism, as Problem 1, the first mechanism at the subject's right; problem 2, the second mechanism at the subject's left (that is, from the end of the series at the subject's left); problem 3, alternately the first mechanism at the subject's right and the first at its left; problem 4, the middle mechanism of the series.

It has become increasingly clear, as our investigations progressed, that the perfect solution of a problem by a given subject is of much less importance as a matter of record than is detailed information concerning the types of reaction and the appearance and disappearance of reactive tendencies during the course of experimentation. For the solution of a problem means simply the termination of a series of observations. It is essential, therefore, that the experimenter fix his attention rather on the immediate response of his subject than on the attainment of the solution of problems. We especially call attention to this matter because many experimenters seem to feel dissatisfied with other than speedy and completely positive results. It seems fair to insist that by the multiple choice method positive results are obtained even if a subject cannot solve any of the problems which are presented to it.

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[4] The results of our experiments, except in the case of the crow, have not been published.

Since it is our intention to more fully discuss the essential features and the technique of the multiple choice method elsewhere, we shall here content ourselves with these brief introductory statements and references. It should perhaps be added that only by reading the earlier article on the behavior of the crow can the reader hope to fully understand the present report.

SUBJECTS

The subjects of the experiments which constitute the observational basis for this paper were two Chester white pigs. They were born April 1st, 1914, and they were therefore two months old when, on June 2nd, they were taken to the Field Station from an adjoining farm and placed in the experimental situation. We shall refer to these individuals as the male and the female, since both sexes were represented. The male, however, had been castrated before we obtained the animals.

From the first, individual differences were conspicuous. The male was considerably smaller and less active and energetic than the female; he ate less and showed less initiative. Throughout the period of observation, both animals were in perfect health and at no time was there reason to suppose that either environmental or physiological conditions were unfavorable to our experiments.

From birth these pigs lived practically out of doors, having a yard to run in and a rather open shelter from storm.

Although the experimenters had expected much of the pigs because of the indications from casual observation of their behavior, it may be said at once that they proved far more satisfactory subjects than we had dared to hope. Indeed, they worked so steadily and uniformly through the investigation that there was practically no loss of time. It is chiefly because of this unexpectedly favorable relation of subject to method that we were enabled to obtain, during the summer of 1914, the numerous results reported below.

APPARATUS

Fortunately, it was possible at the Franklin Field-Station to locate our apparatus in an orchard convenient to the buildings. A rough shelter was built for the pigs under a large apple tree, and convenient yards were arranged by the appropriate use of wire fencing.

The accompanying figures give a fairly good idea of the experimental situation. In figure 1 A, the multiple choice apparatus appears in the foreground, behind a fence which completely surrounds the enclosure. Immediately in front of the apparatus is a bench for the observer. Systems of weighted cords, conspicuous in 1 A, enable the experimenter to operate the slide doors of the multiple choice boxes.

The arrangement of the yards is made clear by figure 1B and figure 2. It was necessary to be able to isolate the pigs for observation as well as to have the apparatus so arranged that an individual could readily be admitted for a trial and on the completion of its reaction, be returned to its appropriate yard.

The multiple choice apparatus proper consists of nine similar boxes, shown in ground plan in figure 2. They were built of rough boards and numbered conspicuously 1 to 9. Each box is sixty inches long, by twenty inches wide, by forty-eight inches deep, with a slide door at each end. The distance between these doors on the inside of the box is forty-eight inches.

From each of the entrance and exit doors a woven window-weight cord extends upward, through a pulley, then horizontally forward through another pulley, and downward, ending in a weight nearly over the observer's bench. To all of the cords from the entrance doors, white weights were attached; to all from exit doors, black weights. Each weight was sufficient to hold its door in position after the latter had been raised. It was found that this required about ten pounds, and iron window weights served our purpose.

In front of the exit door of each box is a v-shaped food trough which is divided into nine like parts by the partitions between

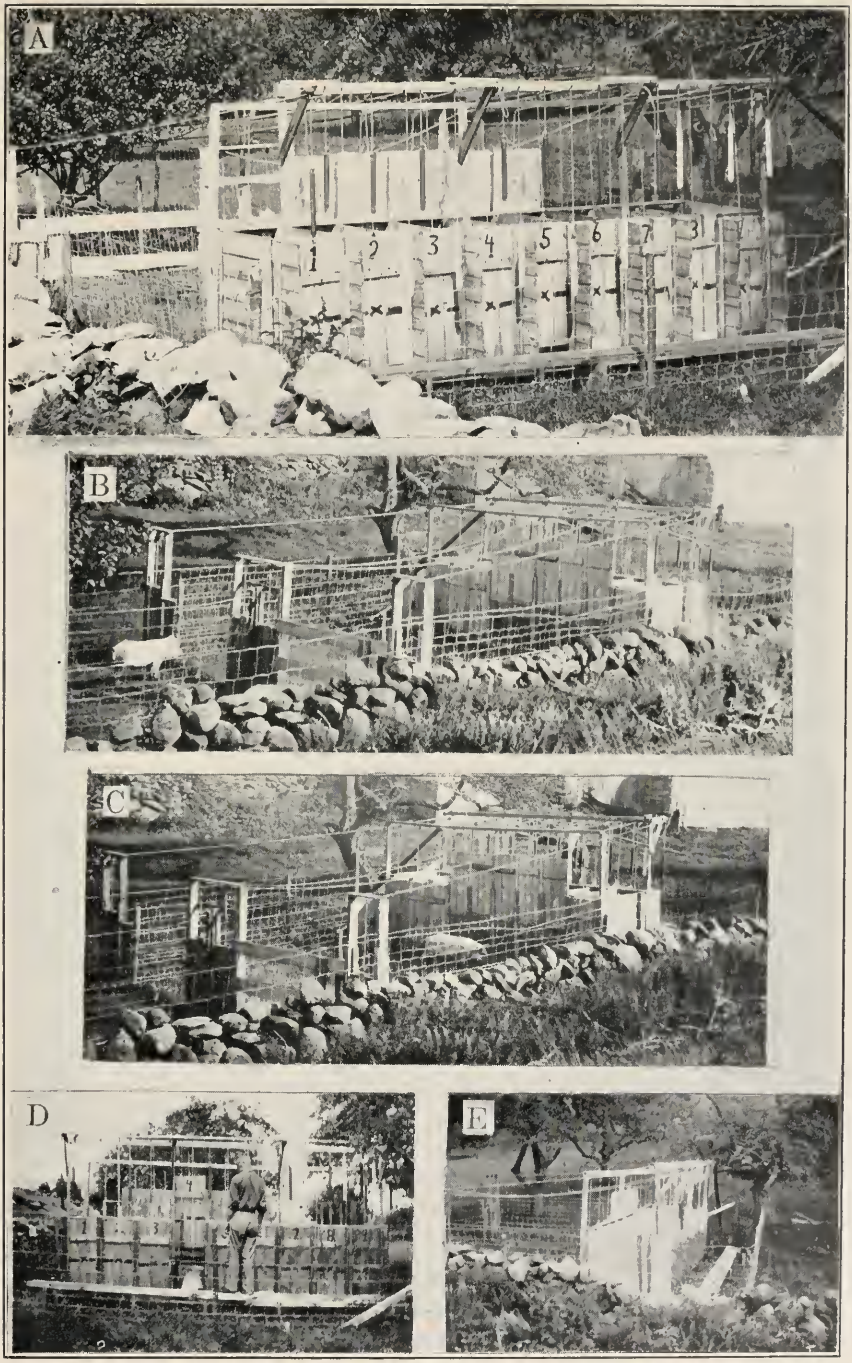

FIGURE 1. Multiple Choice Apparatus for Use with Pigs

A. The reaction-mechanisms from the experimenter's position, showing weighted cords for operating doors. Entrance doors 2 to 6 are raised.

B. The same from the pig's point of view, showing one pig waiting in yard for trial. Entrance doors 2 to 6 raised as in figure A.

C. The same view as that of figure B except that the pig has been admitted to the reaction-space and is about to enter the middle box (no. 4) of those whose doors are open.

D. Here the pig is shown, after appropriate reaction, feeding in the trough of box no. 4. The experimenter appears in the position necessary for manipulation of cords and observation of response.

E. The reaction-mechanisms seen from one end.

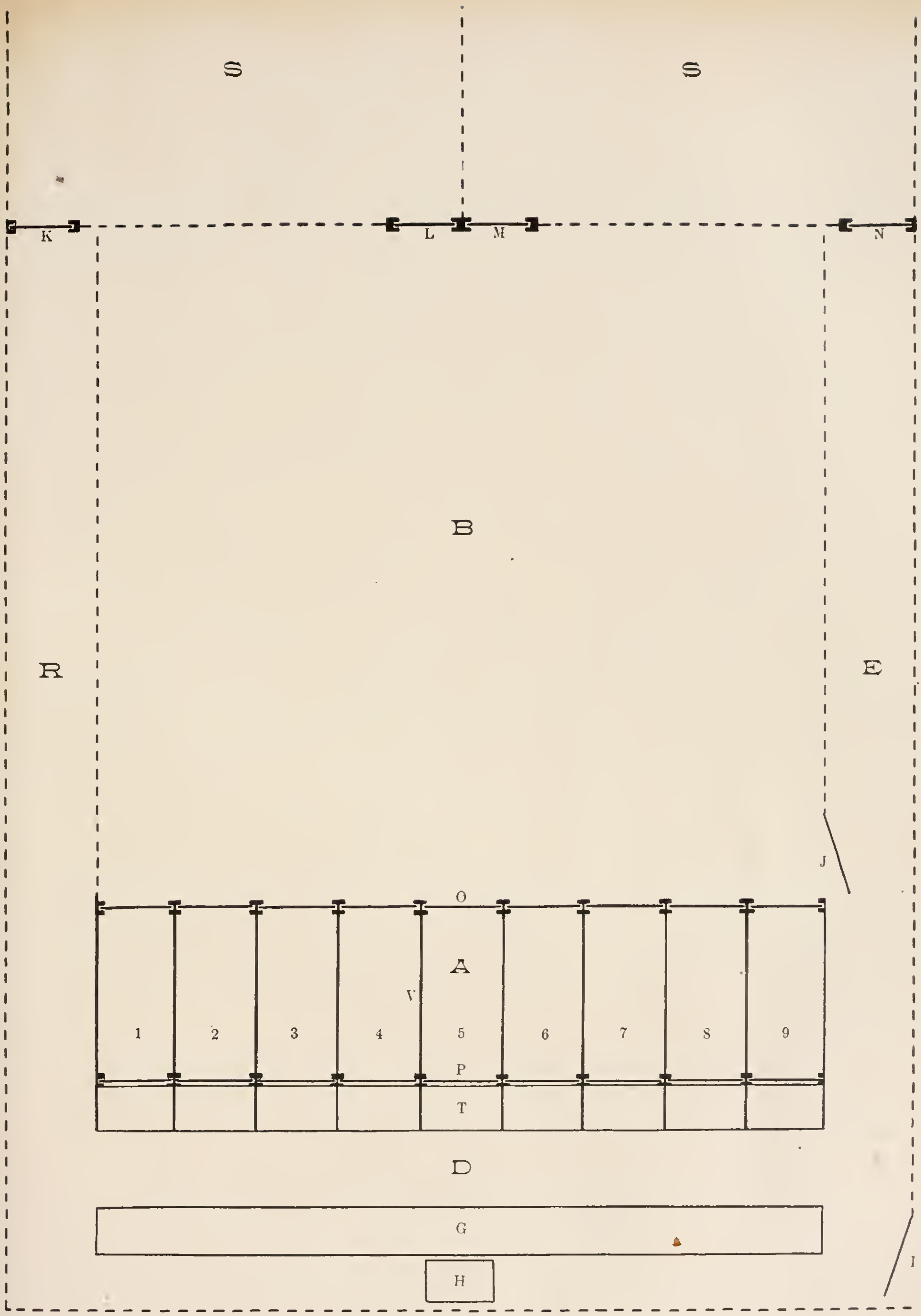

FIGURE 2. Ground Plan of Multiple Choice Apparatus Used for Pigs. Scale $\frac{1}{48}$

A, reaction mechanisms, nine similar boxes or stalls; V, stall number 4; O, entrance door of box; P, exit door of box; T, food trough of box; G, observer's stand and H, writing table; D, runway between trough, T, and stand, G; S, S, yards; B, reaction space; R, E, alleys or runways connecting D with S; I, observer's entrance door to apparatus; J, observer's entrance door to reaction space B; L, M, slide doors between yards and reaction space; K, N, slide doors between yards and alleys.

The weighted cord systems for operating the entrance and exit doors (twenty in all) are not shown in this figure. They may be seen in figure 1, A, B, and C.

boxes. When the exit doors are down, the various parts of the food trough are covered by a horizontally placed sheet of metal which fits closely over them and thus prevents the subjects from obtaining food from the outside of the apparatus.

The large enclosure is divided into four principal parts: (1) the part which contains the reaction-mechanisms with space for the observer's bench, G, and writing table, H, and a passageway for the subject from the exit doors of the apparatus to the yard, S; (2) second, the reaction space which is labelled B in figure 2, in which the subject responded to the multiple choice situation; (3) and finally, the two yards, S, S, from which the subjects started in the case of each trial and to which they returned on the completion of their reaction. K, L, M, and N, designate slide doors between the several portions of the large enclosure, while J and I represent doors which were used by the experimenter.

The entire apparatus was constructed in sections, so that at the end of the season it might readily be taken down and stored.

This brief and very incomplete description will be supplemented somewhat in the section on experimental procedure.

PROBLEMS AND GENERAL METHOD

The four problems enumerated on page 186 were presented to each subject in the order named. For each of these problems, a series of ten settings of the doors was determined upon. These settings differ somewhat from those employed in our study of the crow. It is our intention, so far as possible, to use them with all types of subjects until our observations indicate desirable changes.

We present below for each of the four problems (1) the numbers of the settings, (2) the numbers of the doors open, (3) the total number of doors open in each setting and for the series of ten settings, and (4) the number of the right door.

It was our plan to give each subject an opportunity to respond to each of the ten settings for a given problem in order and to return then to setting 1 and repeat the series. It was found impossible, however, to give ten trials in succession in our early experiments, and in the case of both problems 1 and 2, as a rule a subject was given five trials in succession. For problems 3 and 4 it was found possible to give ten trials in succession.

PROBLEM 1. First Mechanism at the Subject's Right

Settings	Doors open	No. of doors open	No. of right door
1	**1**.2.3	3	1
2	**8**.9	2	8
3	**3**.4.5.6.7	5	3
4	**7**.8.9	3	7
5	**2**.3.4.5.6	5	2
6	**6**.7.8	3	6
7	**5**.6.7	3	5
8	**4**.5.6.7.8	5	4
9	**7**.8.9	3	7
10	**1**.2.3	3	1
	Total	35	

PROBLEM 2. Second Mechanism at the Subject's Left

Settings	Doors open	No. of doors open	No. of right door
1	7.**8**.9	3	8
2	1.2.**3**.4	4	3
3	2.3.4.5.**6.7***	5	6
4	1.2.3.4.**5**.6	6	5
5	4.5.6.**7**.8	5	7
6	1.**2**.3	3	2
7	2.3.**4**.5	4	4
8	1.2.3.4.5.6.7.**8**.9	9	8
9	1.2.**3**.4	4	3
10	3.4.5.6.**7**.8	6	7
	Total	50	

* Changed from 3.4.5.**6**.7 to 2.3.4.5.**6**.7 after about one hundred trials.

PROBLEM 3. Alternately the First Mechanism at Subject's Right and the First at Its Left

Setting	Doors open	No. of doors open	No. of right door
1	**5**.6.7	3	5
2	5.6.**7**	3	7
3	**1**.2.3.4.5.6	6	1
4	1.2.3.4.5.**6**	6	6
5	**4**.5.6.7.8	5	4
6	4.5.6.7.**8**	5	8
7	**2**.3.4.5	4	2
8	2.3.4.**5**	4	5
9	**3**.4.5.6.7.8.9	7	3
10	3.4.5.6.7.8.**9**	7	9
	Total	50	

Problem 4. Middle Mechanism of the Series

Setting	Doors open	No. of doors open	No. of right door
1	2.**3**.4	3	3
2	5.6.**7**.8.9	5	7
3	1.2.3.**4**.5.6.7	7	4
4	7.**8**.9	3	8
5	4.5.**6**.7.8	5	6
6	1.2.3.4.**5**.6.7.8.9	9	5
7	1.**2**.3	3	2
8	2.3.**4**.5.6	5	4
9	3.4.5.**6**.7.8.9	7	6
10	6.**7**.8	3	7
		Total 50	

Both punishment and reward were used in these experiments. The punishment consisted of confinement for a definite interval, usually one minute, in each wrong box entered, while the reward consisted of food which could be obtained in the trough of the right box.

EXPERIMENTAL PROCEDURE

We shall now briefly enumerate, in order to supplement the descriptions of apparatus and methods which have been given, the steps in a regular series of observations.

The experimenter having entered the enclosure with a supply of food, record-book, stop-watch, etc., first raises each of the nine exit doors and places in each section of the trough a quantity of food (sour milk, shelled corn, vegetables). He then lowers the exit doors, thus covering the food, and takes his position on the observation bench. In case both pigs are in the shelter yard, it is next necessary for him to drive one of them into the other yard. This having been done, he may proceed to set the entrance doors for the first trial. Let us suppose that the problem to be presented is problem 4 and that setting 1 is first to be used. In this case the experimenter raises entrance doors 1, 2, and 3. He is now ready to admit one of the pigs to the reaction space B of figure 2. This he does by raising momentarily the appropriate slide door between B and S.

The instant the pig enters the reaction space, the experimenter starts his stop-watch and begins to record the important features of the behavior of the animal, noting especially its approach to the several doors, its tendency to enter boxes and the actual entrance and time of entrance into any one of the three accessible boxes. Let us suppose that the animal enters directly box 3. Immediately the experimenter lowers the entrance door and thus confines the animal in the small compartment as punishment for an incorrect choice. At the expiration of one minute, the entrance door is raised and the pig is allowed to retreat from the box and make another choice. We may now suppose that the animal, after passing in front of boxes 2 and 1, returns to 1 and enters it. The experimenter immediately stops his stop-watch, lowers the entrance door, and, since this box is by definition the right one, he immediately raises the exit door and rewards the animal for correct choice by allowing it to eat for a few seconds. He then, either by speaking to the pig or by touching it with a whip, induces it to pass from the box by way of the passage, D, and the alley, R or E, back to the appropriate yard, S.

Having reset the apparatus, the experimenter now gives the other pig a trial with the same problem and either with the same or with a different setting of the doors.

As a rule, the animals were fed only in the trough of the apparatus. They were almost always hungry, and although sufficiently well fed to keep them growing and in excellent health, they usually seemed fairly hungry at the end of a day's work. In no case was it necessary, in order to induce them to work steadily, to have them extremely hungry.

The influence of visual and olfactory factors was to be expected, and at various points in the investigation, precautions had to be taken against following.

PRELIMINARY TRAINING

On June 2nd the pigs were brought to the Field Station and placed in the shelter yard, and in the afternoon of the same day, they were fed in the trough of the apparatus, all of the doors of the boxes and the yards being raised.

During the next six days they became thoroughly accustomed to the apparatus and learned both to feed in the trough and to make the trip readily from the yards, through the apparatus, and back to the starting point. They very quickly and satisfactorily adapted themselves to the situation, while at the same time becoming thoroughly tame and indifferent to the presence of the experimenter.

On June 9th it seemed fitting to attempt a series of preliminary trials. Each animal was given, in turn, opportunity to secure food in each of the nine boxes. When the subject entered the reaction space, B, the entrance door of a certain box stood open, and as soon as the animal had entered that box, the experimenter closed the door behind it and opened the exit door in front of it, thus enabling it to obtain food. During these preliminary trials, the pigs were in separate yards and were given their trials alternately.

We shall now report the results of our regular experiments.

RESULTS OF EXPERIMENTS

As it is essential to present the data for each trial in the series of experiments, tables 1, 3, 4, 6, 7, 9, and 10 have been constructed after the following manner. At the head of each table stand the several settings, the letter S serving as an abbreviation for setting and the number following it designating the place of the setting in the series. Immediately under the number of the setting appear the numbers of the doors open with the one to be chosen (correct one) printed in bold face type. Below this preliminary information concerning the particular problem in question, appear the results for each of the trials of each subject. The column headed T gives the number of a trial in the total series of trials for a given subject, in a given problem. Following the number of the trial are the numbers of the boxes entered, in the order of entrance. Referring to table 1, we discover that the female in her first trial under problem 1 selected, of the three boxes whose doors were open, first, number 3 She was, of course, punished by being confined in this box for one minute, and on release entered box 1, which was the correct box, and received the reward of food. Or again, in table 3 it may be noted that in trial 146, under problem 2, the female entered, in order, boxes 7, 9, 7, and 8, the group of open doors including 7, 8, and 9, and the box to be entered being number 8.

These tables will enable the reader to obtain quickly definite information concerning the forms of response and the changes therein during the course of experimentation. We shall present the several tables under the problem numbers and reserve further comment for the section on the discussion of results.

DISCUSSION OF RESULTS

The results will now be discussed under the headings of the four problems, and in connection with each a condensed tabular summary of the experiments will be offered, together with such comments as are necessary on the experimental procedure, the behavior of the subjects, and the significance of the various forms of response.

PROBLEM 1

This problem, for which the definition of the correct mechanism is the first at the subject's right, proved extremely easy for the pigs. Incorrect choices were surprisingly few, and the number of trials necessary for the perfect solution of the problem was also surprisingly few for both subjects, the female having chosen correctly throughout a series of ten settings at the end of forty trials and the male having similarly succeeded at the end of forty-five trials.

As is indicated by tables 1 and 2, which contain all of the data for this problem, the experiments were not discontinued at this point, but each individual was given additional opportunity to work out the problem. In the light of our later experience, this was a mistake, but at the time we were unconvinced that the animals were depending upon the relation of the correct mechanism to the other members of the group, and we proceeded further with our observations in order to settle certain points which were in doubt.

From the first it was evident in connection with this problem that the female was more intelligent than the male, and that he tended to be markedly influenced by her. After observations were discontinued with her on June 14th, he reacted very poorly for a number of series, and then again improved and reacted perfectly in the last three series given on June 15th.

In this problem the total number of doors open in the ten settings was, as may be seen by reference to the data presented on page 191, thirty-five. Of these, ten were of course correct. Hence the probability of a correct first choice apart from experience would be 1 to 2.5. In table 2, it appears from the data of the last column for each individual that the ratio of correct to incorrect first choices was on the first day of training 1 to 1 for the female and 1 to 2.33 for the male. It should here be stated that in table 2, as well as in the like tables for the other

TABLE 1

Results for Female in Problem 1

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	1.2.3	T.	8.9	T.	3.4.5.6.7	T.	7.8.9	T.	2.3.4.5.6	T.	6.7.8	T.	5.6.7	T.	4.5.6.7.8	T.	7.8.9	T.	1.2.3
1	3.1	2	8	3	6.3	4	7	5	2	6	7.8.6	7	7.5	8	4	9	7	10	3.1
11	1	12	8	13	7.4.3	14	7	15	2	16	6	17	5	18	4	19	7	20	1
21	2.1	22	8	23	3	24	7	25	3.2										
26	1	27	8	28	5.3	29	5.7	30	2	31	6	32	5	33	4	34	7	35	1
36	1	37	8	38	3	39	7	40	2	41	6	42	6.5	43	4	44	7	45	1
46	1	47	8	48	3	49	7	50	2	51	6	52	5	53	4	54	7	55	1
56	1	57	8	58	3	59	7	60	3.2	61	6	62	5	63	5.4	64	7	65	1
	1.2.3.4.5		5.6.7.8.9		3.4.5.6		7.8.9		2.3.4.5 6.7.8.9										
1	1	2	5	3	3	4	7	5	2										

Results for Male in Problem 1

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
1	2.3.1	2	8	3	7.6.3	4	9.7	5	2	6	8.7.6	7	6.5	8	4	9	9.7	10	2.3.1
11	3.1	12	8	13	7.5.3	14	8.9.7	15	2	16	7.6	17	5	18	6.5.4	19	7	20	3.2.1
21	2.1	22	8	23	6.3	24	7	25	6.4.2										
26	1	27	8	28	3	29	8.7	30	2	31	7.6	32	6.5	33	4	34	7	35	1
36	1	37	8	38	3	39	7	40	2	41	6	42	5	43	4	44	7	45	1
46	1	47	8	48	3	49	7	50	6.2	51	8.6	52	7.7.5	53	8.4	54	8.7	55	1
56	1	57	8	58	3	59	7	60	5.2	61	6	62	5	63	4	64	7	65	3.1
	1.2.3.4.5		5.6.7.8.9		3.4.5.6		7.8.9		2.3.4.5 6.7.8.9										
1	3.1	2	5	3	3	4	9.7	5	4.2										
6	3.1	7	5	8	3	9	9.7	10	5.2										
66	3.1	67	8	68	3	69	8.7	70	2	71	6	72	6.5	73	4	74	7	75	1
76	1	77	8	78	3	79	7	80	2	81	6	82	5	83	4	84	7	85	1
	1.2.3.4.5		5.6.7.8.9		3.4.5.6		7.8.9		2.3.4.5 6.7.8.9										
11	1	12	5	13	3	14	7	15	2										

TABLE 2

Daily Series and Averages with Ratios of Correct to Incorrect First Choices

Problem 1

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 10	1- 5	3	2				June 10	1- 5	2	3			
“	6-10	2	3	5	5	1:1	“	6-10	1	4	3	7	1:2.33
11	11-15	4	1				11	11-15	2	3			
“	16-20	5	0				“	16-20	2	3			
“	21-25	4	1	13	2	1: .15	“	21-25	2	3	6	9	1:1.50
12	26-30	4	1				12	26-30	4	1			
“	31-35	5	0				“	31-35	3	2			
“	36-40	5	0				“	36-40	5	0			
“	41-45	4	1	18	2	1: .11	“	41-45	5	0	17	3	1: .18
13	46-50	5	0				13	46-50	4	1			
“	51-55	5	0				“	51-55	1	4			
“	56-60	4	1				“	56-60	4	1			
“	61-65	4	1	18	2	1: .11	“	61-65	4	1	13	7	1: .54
14	1- 5	5	0	5	0	1:0	14	1- 5	2	3			
							“	6-10	2	3	4	6	1:1.50
							15	66-70	3	2			
							“	71-75	4	1			
							“	76-80	5	0			
							“	81-85	5	0	17	3	1: .18
							“	11-15	5	0	5	0	1:0

problems, the data refer only to first choices in each trial, the column headed R containing the number of correct first choices and that headed W the number of incorrect first choices for each series of trials or for the day. It further appears from this table that five trials constituted the regular series in problem 1, and it should here be stated that the experimenter always resumed experimentation at the point in the regular series of settings at which work had been interrupted. He therefore proceeded in regular order from setting 1 to setting 10 and then returned to setting 1 and repeated the trials.

Further comment on the behavior of the animals in problem 1 is needless, for the task is but slightly more difficult than the acquisition of a simple position habit, and it has already been satisfactorily demonstrated that many of the vertebrates acquire such habits with ease.

PROBLEM 2

For this problem, which is definable as the second mechanism from the subject's left, all of the data for discussion will be found in tables 3, 4, and 5. Again, as in the case of problem 1, the regular series consisted, throughout the training, of five trials, but as many as six such series were given on a single day. Bracketed series appearing, for example, in table 5, under the dates June 23, 24, 25, and 28 and July 1, 2, 3 and 4, were continuous, that is, ten trials were given in succession instead of only five.

For the ten settings of problem 2, the total number of open doors is fifty, and the expectation therefore is that prior to experience an animal will choose correctly once in five times, thus giving a ratio of right to wrong choices of 1 to 4. That this expected ratio does not appear on the first day of experimentation is due to the effect of the previous training in problem 1. The tendency to enter the first box at the left was persistent in both subjects and often that box was re-entered a number of times in spite of punishment. In tables 3 and 4 the data for these statements are presented, and in table 5 it may be noted that on the first day of work on problem 2 neither subject made a single correct first choice.

The ratio of correct to incorrect first choices for the female rapidly, although somewhat irregularly, decreased with experience until on July 4th it stood 1 to .19. On this date she succeeded in choosing correctly in ten successive trials, and was therefore considered to have solved the problem perfectly.

Similarly, the ratio for the male changed fairly rapidly until on July 11th it stood 1 to .11. At this time, although he had not succeeded in choosing correctly in each of the ten settings consecutively, his training was discontinued, for he had already delayed experimentation with the female for a week, and it was perfectly clear that although he made an occasional error, he was capable of perfectly solving the problem.

Whereas the female finished this problem as a result of 390 trials, the male had made only nine out of ten correct choices at the end of 520 trials, when his training was discontinued. We are inclined to think that this is a reliable indication of the difference in docility between these two individuals.

We shall now turn to tables 3 and 4 for a further brief analysis of the reactions.

For about 50 trials in problem 2, both pigs showed the effect of their experience in problem 1. Then the number of correct first choices rapidly increased for each of the ten settings. There were in the case of setting 1 few mistakes on the part of the female after 150 trials, whereas on the part of the male there were more than twice as many incorrect first choices. The same holds in general of each of the other settings, she proving herself much more steady and predictable in response than he. This was doubtless due in a measure to hunger, for it was much more difficult to keep him in the proper condition of eagerness for food than her.

The data of these tables indicate no definite and persistent reactive tendencies during the course of experimentation other than the original acquired tendency to enter the first box at the right in the group and the subsequently acquired tendency to select the second box from the left in the group. Certain of the settings proved very much more difficult than others. Contrary to expectation, difficultness is not directly variable with the number of doors open. Setting 1, for example, as contrasted with setting 6, is much the easier, yet three doors are open in each case. In general, however, it is evidently true that the larger the group of open doors the more difficult it is for the animal to choose correctly and the larger the number of mistakes in a given trial, if the first choice is not correct.

From the behavior of the two pigs in this problem, as contrasted with the first, it is safe to conclude that they are perfectly capable of selecting the proper reaction, mechanism by its relation in a group of similar mechanisms when the number in the group is as large as nine and when the constant relation of the correct mechanism is second from one end. It is further clear that this problem is a much more difficult one for the pigs than problem 1. But it is also certain that the difference in difficultness is not indicated by the difference in the number of experiences necessary for the solution of the problems, since the early days of work on problem 2 served merely to overcome the tendency acquired in connection with problem 1. It seems probable that should we subtract 100 trials from the totals under problem 2 we should have a fair basis of comparison

TABLE 3

Results for Female in Problem 2

T.	S. 1 7.8.9	T.	S. 2 1.2.3.4	T.	S. 3 3.4.5.6.7	T.	S. 4 1.2.3 4.5.6	T.	S. 5 4.5.6.7.8	T.	S. 6 1.2.3	T.	S. 7 2.3.4.5	T.	S. 8 1.2.3.4.5 6.7.8.9	T.	S. 9 1.2.3.4	T.	S. 10 3.4.5 6.7.8
1	7.8	2	1.1.1 1.1.1 7.4.3a†	3	3.7.3.6	4	6.3.6.2 1.6.5a	5	8.4.5.7a	6	3.1.3.3 3.1.2a	7	5.2.5.4a	8	6.4.8	9	4.3a	10	*4.8.8 5.7
11	8	12	3	13	5.7.3.5.6	14	6.1.5	15	6.8.4.7	16	3.1.2	17	2.4	18	2.7.4.8	19	4.1.4.4.3	20	8.7
21	7.8	22	1.4.4 4.3	23	3.6	24	6.3.4 6.3.6 1.4.5	25	5.8.7	26	2	27	2.5.2.5 5.3.4	28	7.1.4.8	29	4.3	30	7
31	7.8	32	4.2.3	33	7.6	34	5	35	6.8.5.8 6.4.8.7	36	1.2	37	5.4	38	6.8	39	4.2.4 1.4.3	40	8.6.3 5.8.6.4 8.3.7
41	7.9.8	42	4.2.4 1.3	43	7.6	44	5	45	8.6.5.4 8.5.7	46	3.3.2	47	5.4	48	7.9.6.7 5.8	49	3	50	8.7
51	8	52	3	53	6	54	6.2.5	55	7	56	3.3.2	57	5.2.4	58	8	59	4.3	60	7
61	8	62	2.4.1.3	63	7.6	64	6.5	65	8.4.5.7										
66	7.8	67	3	68	7.5.7 5.6	69	1.6.2 6.5	70	8.7	71	2	72	4	73	7.9.3 6.2.8	74	4.3	75	5.7
76	7.9.7.8	77	4.3	78	7.6	79	6.5	80	8.5.8.4.7	81	1.3.2	82	4	83	7.8	84	2.4.3	85	7
86	7.8	87	2.4.3	88	7.6	89	6.5	90	8.7	91	3.2	92	2.5.4	93	7.8	94	4.2.3	95	8.7
96	8	97	3	98	7.6	99	6.5	100	8.5.7	101	3.2	102	5.4	103	8	104	2.3	105	8.6.7
106	7.8	107	4.3	108	7.6*	109	3.6.6.4.5	110	8.5.7	111	1.2	112	5.5.4	113	8	114	3	115	8.6.7
116	7.7.8	117	2.4.3	118	5.6	119	3.5	120	8.8.7	121	3.2	122	2.5.4	123	7.8	124	3	125	8.7
126	7.8	127	3	128	7.6	129	3.5	130	5.7	131	3.2	132	2.4	133	7.7.4.8	134	3	135	3.8.6.7
136	7.8	137	3	138	6	139	2.3.4.5	140	5.7	141	2	142	2.4	143	3.7.6 3.5.8	144	4.3	145	5.8.7
146	7.9.7.8	147	4.3	148	3.6	149	5	150	7	151	3.2	152	5.4	153	8	154	4.2.3	155	8.5.7
156	7.8	157	3	158	7.6	159	6.5	160	5.8.7	161	3.2	162	4	163	8	164	3	165	8.7
166	7.8	167	3	168	5.6	169	5	170	7	171	2	172	4	173	8	174	3	175	8.7
176	8	177	3	178	4.6	179	3.5	180	8.7	181	2	182	5.3.3.4	183	7.8	184	3	185	7
186	8	187	3	188	7.6	189	3.5	190	7	191	3.2	192	5.4	193	8	194	3	195	7
196	8	197	2.3	198	7.6	199	3.5	200	8.7	201	3.2	202	5.5.4	203	8	204	3	205	7

206	8	207	3	208	7.6	209	2.5	210	8.7	211	1.3.2	212	4	213	8	214	3	215	8.7
216	8	217	3	218	7.6	219	3.5	220	8.7	221	2	222	5.3.4	223	7.7.7.8	224	3	225	8.6.7
226	8	227	4.3	228	6	229	6.5	230	8.7	231	3.2	232	4	233	8	234	3	235	3.6.7
236	8	237	3	238	7.6	239	6.5	240	8.7	241	2	242	4	243	8	244	3	245	7
246	8	247	3	248	6	249	4.3.5	250	7	251	2	252	4	253	8	254	3	255	7
256	8	257	3	258	7.6	259	3.5	260	8.7	261	3.2	262	5.3.2.5.4	263	8	264	3	265	7
266	8	267	3	268	6	269	5	270	8.6.7	271	2	272	3.5.4	273	8	274	4.3	275	7
276	8	277	4.3	278	6	279	6.5	280	8.7	281	3.2	282	4	283	8	284	3	285	7
286	8	287	4.2.3	288	6	289	5	290	7	291	3.2	292	4	293	7.8	294	4.3	295	7
296	8	297	3	298	6	299	5	300	7	301	3.2	302	5.4	303	8	304	3	305	8.7
306	8	307	4.3	308	7.6	309	5	310	7	311	3.2	312	4	313	8	314	3	315	7
316	8	317	3	318	6	319	5	320	7	321	3.2	322	5.4	323	7.8	324	3	325	7
326	8	327	3	328	6	329	5	330	7	331	2	332	4	333	7.6.8	334	3	335	7
336	8	337	4.3	338	6	339	5	340	7	341	3.2	342	4	343	8	344	3	345	7
346	8	347	3	348	6	349	5	350	7	351	2	352	3.2.2.5.4	353	7.6.8	354	4.3	355	7
356	8	357	3	358	6	359	6.5	360	7	361	3.2	362	5.4	363	8	364	3	365	7
366	8	367	3	368	6	369	4.5	370	7	371	3.2	372	4	373	7.8	374	3	375	7
376	7.8	377	3	378	6	379	5	380	7	381	2	382	4	383	8	384	3	385	7
386	8	387	3	388	6	389	5	390	7										
391	8	392	3	393	6	394	5	395	6.7	396	2	397	4	398	8	399	3	400	7

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* Setting 3 was changed in this series and the succeeding series from 3.4.5.6.7 to 2.3.4.5.6.7 in order to make the ratio of right doors to wrong doors in the series of ten settings 10:40.

† The letter "a" indicates that the pig was aided in choosing the right door.

TABLE 4

Results for Male in Problem 2

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	7.8.9	T.	1.2.3.4	T.	3.4.5.6.7	T.	1.2.3 4.5.6	T.	4.5.6.7.8	T.	1.2.3	T.	2.3.4.5	T.	1.2.3.4.5 6.7.8.9	T.	1.2.3.4	T.	3.4.5 6.7.8
1	7.7.7 7.7.7 9.7.8	2	1.4.1.3	3	3.3.6	4	1.5	5	6.4.7	6	1.1.2	7	5.2.5.2 5.3.5.4	8	1.5.8	9	4.1.4.2 1.4.1 2.4.1.3	10	8.7
11	8	12	4.1.2 4.3	13	7.6	14	6.4.1 6.2.4 6.3.5	15	7	16	3.1.3 1.3.2	17	5.2.5.3 2.5.4	18	7.9.8	19	3	20	8.7
21	8	22	2.4.1 4.3	23	7.6	24	6.5	25	8.8.7	26	1.3.2	27	2.5.5.4	28	8	29	4.2.1 4.2.3	30	8.7
31	8	32	3	33	7.3.4.3.6	34	6.5	35	8.4.6.4.7	36	1.3.2	37	5.2.4	38	9.6.8	39	3	40	8.3.7
41	8	42	4.2.3	43	7.6	44	6.6.3.2 4.3.5	45	8.7	46	3.2	47	5.4	48	7.6.4.3 2.1.9.8	49	3	50	8.7
51	8	52	4.3	53	7.6	54	5	55	8.7	56	1.3.2	57	5.4	58	7.8	59	3	60	8.7
61	8	62	3	63	6	64	3.6.5	65	4.8.7										
66	8	67	3	68	7.6	69	4.3.5	70	8.7	71	2	72	5.3.2.5.4	73	8	74	4.3	75	8.7
76	9.7.8	77	4.3	78	3.3.7.6	79	1.3.2.5	80	8.7	81	3.2	82	5.2.2.4	83	8	84	4.3	85	7
86	8	87	3	88	7.6	89	5	90	8.7	91	2	92	5.4	93	8	94	3	95	6.5.4 3.8.7
96	7.7.9.8	97	3	98	5.3.7.6	99	2.5	100	4.8.7	101	2	102	4	103	7.6.4.8	104	4.2.3	105	8.7
106	9.8	107	4.3	108	7.6*	109	4.3.2.5	110	8.7	111	3.2	112	5.4	113	8	114	3	115	8.7
116	8	117	3	118	6	119	5	120	8.7	121	3.2	122	5.4	123	7.6.5 4.3.8	124	3	125	8.7
126	8	127	4.3	128	7.6	129	5	130	8.6.5.7	131	2	132	3.4	133	8	134	3	135	8.7
136	8	137	2.4.3	138	6	139	6.5	140	8.7	141	2	142	5.4	143	8	144	3	145	3.7
146	8	147	3	148	6	149	5	150	7	151	3.2	152	5.4	153	8	154	3	155	8.7
156	8	157	2.1.4.3	158	6	159	5	160	7	161	3.2	162	5.2.5.5.4	163	7.6.8	164	3	165	7
166	8	167	3	168	6	169	5	170	7	171	3.3.2	172	5.3.2.5.4	173	8	174	3	175	8.7
176	8	177	3	178	5.4.3.7.6	179	2.3.5	180	8.7	181	2	182	4	183	8	184	3	185	5.7
186	7.8	187	3	188	7.3.6	189	4.5	190	6.8.7	191	2	192	4	193	8	194	4.3	195	8.7
196	8	197	4.3	198	7.6	199	2.3.5	200	8.7	201	3.2	202	5.4	203	8	204	3	205	7
206	8	207	3	208	7.6	209	5	210	8.7	211	3.2	212	5.4	213	8	214	3	215	8.7
216	7.8	217	3	218	5.4.7.6	219	5	220	8.7	221	2	222	5.4	223	8	224	3	225	8.7

226	8	227	4.3	228	7.6	229	5	230	8.7	231	3.2	232	5.4	233	8	234	3	235	7
236	8	237	3	238	6	239	5	240	8.7	241	3.2	242	5.4	243	7.6.4.3 2.9.8	244	3	245	7
246	8	247	3	248	6	249	5	250	8.7	251	3.2	252	5.4	253	8	254	3	255	5.4.3.7
256	8	257	3	258	5.4.3.2.6	259	5	260	8.6.5.4.7	261	2	262	4	263	8	264	3	265	6.5.4.3.7
266	8	267	2.3	268	6	269	5	270	7	271	3.2	272	5.4	273	8	274	4.2.3	275	7
276	7.8	277	3	278	3.2.5 4.2.6	279	6.5	280	7	281	2	282	3.4	283	8	284	3	285	7
286	7.8	287	3	288	5.6	289	3.4.6.5	290	7.5.7	291	2	292	2.3.5.4	293	7.6.8	294	3	295	5.6.5.8.7
296	8	297	3	298	5.4.3.7.6	299	5	300	7	301	2	302	5.4	303	8	304	2.3	305	7
306	8	307	3	308	7.6	309	5	310	5.4.8.7	311	3.2	312	5.4	313	8	314	3	315	7
316	8	317	3	318	5.6	319	4.6.2.5	320	7	321	2	322	4	323	7.6.5.4 3.2.7.8	324	3	325	4.8.7
326	8	327	3	328	6	329	4.3.2.1.5	330	7	331	2	332	5.4	333	8	334	3	335	6.5.7
336	8	337	3	338	6	339	5	340	4.6.5.7	341	3.2	342	5.4	343	8	344	4.3	345	7
346	7.9.8	347	3	348	5.4.5.6	349	5	350	5.5.7	351	2	352	3.4	353	8	354	3	355	5.6.8.7
356	8	357	4.3	358	7.6	359	6.5	360	7	361	3.2	362	5.4	363	8	364	3	365	7
366	8	367	4.3	368	6	369	5	370	7	371	3.2	372	3.5.4	373	8	374	3	375	7
376	8	377	4.3	378	6	379	6.5	380	7	381	3.2	382	2.4	383	8	384	3	385	7
386	8	387	4.3	388	7.6	389	5	390	7										
391	8	392	3	393	6	394	5	395	7	396	3.2	397	5.4	398	8	399	2.3	400	7
401	8	402	4.3	403	7.6	404	5	405	7	406	3.2	407	4	408	7.6.5.8	409	2.4.3	410	8.4.3.6 5.8.6.7
411	7.8	412	3	413	6	414	4.5	415	7	416	3.2	417	5.4	418	7.6.8	419	2.3	420	7.6.8.5.7
421	8	422	3	423	7.5.3 2.6	424	5	425	7										
426	8	427	4.3	428	7.6	429	5	430	8.7	431	3.2	432	5.4	433	8	434	4.3	435	8.7
436	8	437	3	438	5.7.6	439	3.2.1.5	440	7	441	3.2	442	5.4	443	7.6.8	444	3	445	7
446	8	447	3	448	6	449	5	450	8.7	451	3.2	452	5.3.4	453	8	454	3	455	7
456	8	457	3	458	6	459	5	460	7	461	3.2	462	5.4	463	7.6.8	464	3	465	7
466	8	467	3	468	5.4.7.6	469	5	470	7										
471	8	472	3	473	6	474	5	475	7	476	3.2	477	5.4	478	7.6.8	479	3	480	6.5.7
481	7.8	482	3	483	6	484	4.3.2.5	485	6.5.7	486	3.2	487	5.4	488	7.5.8	489	3	490	5.7
491	8	492	3	493	6	494	3.2.5	495	6.4.7	496	2	497	3.4	498	7.8	499	3	500	7
501	8	502	3	503	6	504	5	505	6.7	506	3.2	507	5.4	508	7.8	509	3	510	7
511	8	512	3	513	6	514	5	515	7	516	2	517	4	518	7.8	519	3	520	7

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* Setting 3 was changed in this series and the succeeding ones from 3.4.5.**6**.7 to 2.3.4.5.**6**.7 in order to make the ratio of right doors to wrong doors in the series of ten settings 10:50.

TABLE 5

Daily Series and Averages with Ratios of Correct to Incorrect First Choices

Problem 2

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June							June						
16	1- 5	0	5				16	1- 5	0	5			
"	6-10	0	5	0	10	0:1	"	6-10	0	5	0	10	0:1
17	11-15	2	3				17	11-15	2	3			
"	16-20	0	5				"	16-20	1	4			
"	21-25	0	5	2	13	1:6.50	"	21-25	1	4	4	11	1:2.75
18	26-30	2	3				18	26-30	1	4			
"	31-35	1	4				"	31-35	2	3			
"	36-40	0	5				"	36-40	1	4			
"	41-45	1	4	4	16	1:4.00	"	41-45	1	4	5	15	1:3.00
19	46-50	1	4				19	46-50	1	4			
"	51-55	4	1				"	51-55	2	3			
"	56-60	2	3				"	56-60	1	4			
"	61-65	1	4	8	12	1:1.50	"	61-65	3	2	6	9	1:1.50
20	66-70	1	4				20	66-70	2	3			
"	71-75	2	3				"	71-75	2	3			
"	76-80	0	5				"	76-80	0	5			
"	81-85	2	3	5	15	1:3.00	"	81-85	2	3	6	14	1:2.33
21	86-90	0	5				21	86-90	3	2			
"	91-95	0	5				"	91-95	3	2			
"	96-100	2	3				"	96-100	1	4			
"	101–	1	4	3	17	1:5.67	"	101–	2	3	9	11	1:1.22
22	106–	0	5				22	106–	0	5			
"	111–	2	3				"	111–	2	3			
"	116–	0	5				"	116–	4	1			
"	121–	1	4	3	17	1:5.67	"	121–	1	4	7	13	1:1.86
23	126–	1	4				23	126–	2	3			
"	131–	1	4				"	131–	3	2			
"	136–	2	3				"	136–	2	3			
"	141–	1	4				"	141–	3	2			
"	146–	2	3	7	18	1:2.57	"	146–	5	0	15	10	1: .67
24	151–	1	4				24	151–	2	3			
"	156–	1	4				"	156–	4	1			
"	161–	3	2				"	161–	2	3			
"	166–	3	2				"	166–	5	0			
"	171–	4	1	12	13	1:1.08	"	171–	2	3	15	10	1: .67
25	176–	2	3				25	176–	2	3			
"	181–	3	2				"	181–	4	1			
"	186–	3	2				"	186–	1	4			
"	191–	3	2	11	9	1: .82	"	191–	3	2	10	10	1:1
26	196–	1	4				26	196–	1	4			
"	201–	3	2				"	201–	3	2			
"	206–	2	3	6	9	1:1.50	"	206–	3	2	7	8	1:1.14
27	211–	3	2				27	211–	2	3			
"	216–	2	3				"	216–	2	3			
"	221–	2	3	7	8	1:1.14	"	221–	3	2	7	8	1:1.14
28	226–	2	3				28	226–	2	3			
"	231–	3	2				"	231–	3	2			
"	236–	2	3				"	236–	4	1			
"	241–	5	0				"	241–	2	3			
"	246–	4	1				"	246–	4	1			
"	251–	5	0	21	9	1: .43	"	251–	2	3	17	13	1: .76
29	256–	2	3				29	256–	3	2			

TABLE 5—*Continued*

DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT FIRST CHOICES

PROBLEM 2

Female

Date	No. of trials	R	W	R	W	Ratio of R to W
June 29	261–	3	2			
“	266–	4	1			
“	271–	3	2			
“	276–	2	3			
“	281–	4	1	18	12	1: .67
30	286–	4	1			
“	291–	2	3			
“	296–	5	0			
“	301–	2	3	13	7	1: .54
July 1	306–	3	2			
“	311–	4	1			
“	316–	5	0			
“	321–	2	3	14	6	1: .43
2	326–	5	0			
“	331–	4	1			
“	336–	4	1			
“	341–	4	1	17	3	1: .18
3	346–	5	0			
“	351–	2	3			
“	356–	4	1			
“	361–	3	2	14	6	1: .43
4	366–	4	1			
“	371–	3	2			
“	376–	4	1			
“	381–	5	0			
“	386–	5	0	21	4	1: .19
11	391	4	1			
“	396	5	0	9	1	1: .11

Male

Date	No. of trials	R	W	R	W	Ratio of R to W
June 29	261–	4	1			
“	266–	4	1			
“	271–	2	3			
“	276–	2	3			
“	281–	4	1	19	11	1: .58
30	286–	1	4			
“	291–	2	3			
“	296–	4	1			
“	301–	3	2	10	10	1:1
July 1	306–	3	2			
“	311–	3	2			
“	316–	3	2			
“	321–	2	3	11	9	1: .82
2	326–	4	1			
“	331–	3	2			
“	336–	4	1			
“	341–	2	2	13	7	1: .54
3	346–	2	3			
“	351–	3	2			
“	356–	2	3			
“	361–	3	2	10	10	1:1
4	366–	4	1			
“	371–	3	2			
“	376–	3	2			
“	381–	3	2			
“	386–	3	2	16	9	1: .56
5	391–	5	0			
“	396–	2	3			
“	401–	3	2			
“	406–	1	4			
“	411–	3	2	14	11	1: .79
6	416–	0	5			
“	421–	4	1	4	6	1:1.50
7	426–	2	3			
“	431–	1	4			
“	436–	3	2			
“	441–	2	3	8	12	1:1.50
8	446–	4	1			
“	451–	3	2			
“	456–	5	0			
“	461–	2	3			
“	466–	4	1	18	7	1: .39
9	471–	5	0			
“	476–	1	4			
“	481–	2	3			
“	486–	1	4	9	11	1:1.22
10	491–	3	2			
“	496–	3	2			
“	501–	4	1			
“	506–	2	3	12	8	1: .67
11	511–	5	0			
“	516–	4	1	9	1	1: .11

with problem 1. It would then appear to be from four to eight times as difficult as the latter.

One important aspect of the experiment should be here considered. According to our procedure, one of the pigs led and the other followed in a series of trials. It was therefore possible that the follower might be aided in its choice either by watching its companion or by the odor of the box in which the animal fed. There can be no doubt of the tendency of the pigs both to watch one another and to be influenced by the odor of the boxes, but that the solution of the problems did not depend upon either of these factors, although the number of trials necessary to solution may have been modified thereby, is proved by the fact that both subjects made ninety per cent of correct choices when leading.

PROBLEM 3

All of the data in connection with this problem are to be found in tables 6, 7, and 8. The problem is definable as alternately the first mechanism at the right and the first at the left. At the beginning of work on this problem, the animals were given their trials alternately as in the preceding problems, but a strong tendency to follow manifested itself, and on the second day the trials were given by pairs. That is, each individual was allowed to choose in succession the first door at its right and the first door at its left, and was then required to wait while its companion responded to the same pair of settings. Thus, following was rendered impossible.

The tendency to choose the second door from the left naturally manifested itself in the early work on this problem, but it was soon destroyed by training, and the course of experimentation proceeded smoothly to the perfect solution of the problem.

It is to be noted that from the first, ten trials constituted a series. Because of the familiarity with the general experimental situation which the animals had acquired and the experience of the experimenters in the control of hunger and punishment, it was easier to obtain reactions to ten successive trials at this time in the investigation than to five early in the work, with problems 1 or 2.

The female succeeded in solving problem 3 as the result of 420 trials; the male, as the result of 470.

For this problem as for problem 2, the expectation prior to experience is one correct first choice to four incorrect first choices. The male in his first series exhibited exactly this ratio, whereas the female gave a ratio of 1 to 1. Her success, however, was undoubtedly due to following, for in immediately subsequent trials when following was rendered impossible by the giving of the trials by pairs, she did very poorly. The daily ratios for each individual, as presented in table 8, are of interest, but they are by no means as important as are the detailed data of tables 6 and 7.

As might have been expected, after the previously acquired tendency to select the first mechanism at the left had been overcome, the pigs shortly exhibited the tendency to select the end boxes, and they then had to overcome the difficulty of selecting the right end. It is quite possible that this task was rendered easier by the rhythm which resulted from the giving of trials by pairs, but it was perfectly evident from control experiments that the animals could choose correctly even if given their trials in rapid succession, without the irregularity due to alternate experimenting with the two individuals.

Since it seemed possible that the animals might have learned the proper settings and be responding to definite situations rather than to the relation of the right box to the other members of the group, a control experiment was made by the presentation of a new series of settings. At the bottoms of tables 6 and 7 appear the results of these control observations.

The female had solved problem 3 on the completion of trial 420 (see tables 6 and 8), and the male on the completion of trial 470 (see tables 7 and 8). The next series of ten trials for each was preliminary to the control experiments and served also as a demonstration series to certain other observers. Following this demonstration in which both pigs reacted fairly well, the series of settings indicated in tables 6 and 7 was presented. Both individuals were somewhat disturbed by the change, her record being seven correct choices out of ten, and his nine out of ten. Later in the day another series of ten trials, according to the original settings, was given with the result that the female made three incorrect first choices in ten and the male two. Still later, the control settings were again presented. This time she chose correctly eight times in ten and he only five times.

TABLE 6

Results for Female in Problem 3

T.	S. 1 **5**.6.7	T.	S. 2 5.6.**7**	T.	S. 3 **1**.2.3 4.5.6	T.	S. 4 1.2.3 4.5.**6**	T.	S. 5 **4**.5.6.7.8	T.	S. 6 4.5.6.7.**8**	T.	S. 7 **2**.3.4.5	T.	S. 8 2.3.4.**5**	T.	S. 9 **3**.4.5.6 7.8.9	T.	S. 10 3.4.5.6 7.8.**9**
1	7.5	2	6.5.7	3	3.5.4.6 5.3.2.5 4.3.6.5 4.1	4	6	5	4	6	8	7	2	8	3.4.3.5	9	8.7.6.7 5.8.4.7.9 5.8.6 7.3	10	9
11	7.6.5	12	6.7	13	5.6.4 2.3.1	14	5.4.3.6	15	7.5.6.4	16	7.8	17	4.2	18	3.5	19	8.7.6.3	20	4.7.8.9
21	7.6.5	22	5.7	23	2.1	24	3.6	25	8.7.6.5.4	26	8	27	2	28	5	29	9.7.3	30	7.8.7.6.9
31	7.6.7.5	32	5.7	33	4.2.1	34	3.6	35	8.4	36	4.7.5.4.8	37	3.2	38	5	39	8.7.3	40	6.5.8.7.9
41	5	42	5.6.7	43	2.1	44	3.4.6	45	4	46	6.8	47	3.2	48	5	49	9.5.3	50	8.6.3 8.4.9
51	7.6.5	52	5.7	53	2.3.1	54	6	55	8.4	56	6.4.5.7.8	57	2	58	5	59	3	60	7.8.9
61	5	62	7	63	3.2.6.5.1	64	4.6	65	5.7.8.4	66	6.4.6.8	67	2	68	3.5	69	6.9.3	70	4.8.6.9
71	5	72	7	73	5.3.1	74	6	75	8.4	76	7.8	77	2	78	3.5	79	9.3	80	8.9
81	5	82	5.7	83	6.1	84	3.5.6	85	8.5.8.5.4	86	7.8	87	2	88	3.5	89	9.3	90	7.9
91	6.7.5	92	6.7	93	2.6.4 5.6.1	94	3.6	95	6.8.4	96	7.4.8	97	2	98	5	99	7.9.8.9.3	100	7.8.9
101	5	102	7	103	2.5.6.1	104	4.6	105	6.8.4	106	6.8	107	2	108	5	109	8.9.6.9 7.9.3	110	3.7.8.9
111	7.5	112	6.7	113	1	114	3.6	115	8.7.4	116	8	117	5.3.2	118	5	119	7.8.9.3	120	7.9
121	7.5	122	7	123	2.6.1	124	4.1.3.6	125	8.4	126	6.8	127	2	128	5	129	8.9.3	130	8.9
131	6.7.5	132	7	133	1	134	6	135	7.8.4	136	6.8	137	2	138	5	139	9.8.9.3	140	7.9
141	5	142	7	143	1	144	3.6	145	8.4	146	7.8	147	2	148	5	149	3	150	8.9
151	6.7.5	152	7	153	1	154	3.6	155	7.8.4	156	6.8	157	2	158	5	159	9.3	160	9
161	7.5	162	6.7	163	1	164	3.4.6	165	4	166	8	167	2	168	3.5	169	8.3	170	7.9
171	5	172	7	173	1	174	3.6	175	8.4	176	7.8	177	2	178	5	179	9.8.3	180	9
181	6.5	182	7	183	2.4.6.1	184	3.1.6	185	4	186	8	187	2	188	3.2.5	189	9.3	190	6.9
191	5	192	7	193	2.4.6.1	194	3.6	195	8.4	196	7.8	197	3.5.2	198	5	199	9.3	200	6.9
201	5	202	7	203	2.6.1	204	6	205	8.4	206	6.8	207	2	208	5	209	9.3	210	9

211	6.7.5	212	7	213	6.1	214	6	215	{7.8.5 7.8.4	216	8	217	3.5.2	218	5	219	9.3	220	9
221	5	222	7	223	2.6.1	224	6	225	4	226	8	227	2	228	5	229	9.3	230	9
231	5	232	7	233	2.1	234	6	235	4	236	7.5.4.8	237	2	238	3.5	239	3	240	9
241	5	242	5.7	243	1	244	6	245	4	246	8	247	2	248	5	249	3	250	9
251	5	252	7	253	1	254	2.6	255	4	256	8	257	2	258	3.5	259	3	260	9
261	5	262	7	263	1	264	6	265	4	266	8	267	2	268	5	269	9.3	270	9
271	5	272	7	273	1	274	3.6	275	4	276	8	277	2	278	4.5	279	3	280	9
281	5	282	5.7	283	1	284	3.6	285	8.4	286	8	287	2	288	5	289	3	290	9
291	5	292	7	293	1	294	3.6	295	8.4	296	8	297	2	298	5	299	9.3	300	9
301	5	302	7	303	1	304	6	305	8.4	306	7.8	307	2	308	3.5	309	3	310	9
311	5	312	7	313	1	314	6	315	8.4	316	8	317	2	318	5	319	3	320	9
321	7.5	322	7	323	1	324	5.6	325	4	326	8	327	2	328	4.5	329	9.3	330	9
331	5	332	7	333	1	334	6	335	4	336	8	337	2	338	3.5	339	3	340	9
341	6.5	342	7	343	2.1	344	6	345	4	346	8	347	2	348	5	349	9.3	350	8.9
351	5	352	7	353	1	354	3.6	355	4	356	8	357	2	358	5	359	3	360	9
361	5	362	7	363	1	364	6	365	4	366	8	367	2	368	5	369	9.3	370	9
371	6.5	372	7	373	1	374	6	375	4	376	8	377	2	378	5	379	3	380	9
381	5	382	7	383	1	384	4.6	385	4	386	8	387	2	388	4.5	389	9.3	390	9
391	5	392	7	393	1	394	6	395	8.4	396	8	397	2	398	5	399	3	400	9
401	6.5	402	7	403	2.1	404	6	405	4	406	8	407	2	408	5	409	3	410	9
411	5	412	7	413	1	414	6	415	4	416	8	417	2	418	5	419	3	420	9
421	5	422	6.7	423	1	424	6	425	5.4	426	7.8	427	2	428	5	429	3	430	9
	3.4.5.6		3.4.5.**6**		**4**.5.6 7.8.9		4.5.6 7.8.**9**		**1**.2.3.4.5		1.2.3.4.**5**		**2**.3.4 5.6.7		2.3.4 5.6.**7**		**3**.4.5 6.7.8		3.4.5 6.7.**8**
1	4.6.3	2	6	3	4	4	9	5	2.1	6	5	7	7.2	8	7	9	3	10	8
431	5	432	7	433	1	434	6	435	6.8.4	436	7.8	437	2	438	5	439	9.3	440	9
	3.4.5.6		3.4.5.**6**		**4**.5.6 7.8.9		4.5.6 7.8.**9**		**1**.2.3.4.5		1.2.3.4.**5**		**2**.3.4 5.6.7		2.3.4 5.6.**7**		**3**.4.5 6.7.8		3.4.5 6.7.**8**
11	3	12	6	13	5.9.4	14	9	15	1	16	5	17	2	18	7	19	5.3	20	8

TABLE 7

Results for Male in Problem 3

T.	S. 1 **5**.6.7	T.	S. 2 5.6.**7**	T.	S. 3 **1**.2.3 4.5.6	T.	S. 4 1.2.3 4.5.**6**	T.	S. 5 **4**.5.6.7.8	T.	S. 6 4.5.6.7.8	T.	S. 7 **2**.3.4.5	T.	S. 8 2.3.4.**5**	T.	S. 9 **3**.4.5.6 7.8.9	T.	S. 10 3.4.5.6 7.8.9
1	6.5	2	7	3	5.4.3.2 6.5.3 4.2.5 3.5.1	4	1.3.5.5.6	5	7.6.4	6	7.6.8	7	5.4.3.2	8	5	9	8.7.6.8 7.7.3	10	5.7.8.9
11	6.5	12	6.7	13	2.1	14	5.4.3.6	15	7.8.5.4	16	8	17	2	18	5	19	3	20	8.7.9
21	6.5	22	7	23	5.4.3.2 6.5.1	24	3.2.6	25	8.7.4	26	6.8	27	3.5.2	28	3.5	29	8.7.9 8.6.3	30	8.7.5 4.8.9
31	7.6.5	32	7	33	5.4.3.2.1	34	5.4.3.5.6	35	8.6.8.7.4	36	8	37	3.2	38	2.5	39	7.6.9 8.7.3	40	8.9
41	7.6.5	42	7	43	5.3.2.4.1	44	5.2.1.4.6	45	7.8.6.7.4	46	8	47	5.2	48	2.4.3.2.5	49	9.7.6.8 9.8.5.4 8.7.4.6 8.3	50	9
51	6.7.5	52	5.7	53	3.1	54	2.5.6	55	8.6.4	56	8	57	3.5.2	58	4.5	59	9.8.3	60	6.3.8.7.9
61	7.5	62	6.5.7	63	3.1	64	2.6	65	7.8.7.4	66	8	67	4.2	68	3.5	69	9.3	70	7.9
71	7.5	72	6.7	73	5.1	74	2.1.4.2.6	75	4	76	6.8	77	5.2	78	3.5	79	9.5.3	80	9
81	7.5	82	6.7	83	1	84	3.4.6	85	8.7.8.4	86	7.8	87	2	88	3.5	89	9.3	90	9
91	6.5	92	7	93	3.1	94	6	95	8.4	96	8	97	3.5.2	98	5	99	9.4.8.9 4.8.9.3	100	6.9
101	5	102	7	103	4.1	104	6	105	8.5.8 6.8.4	106	5.8	107	3.2	108	5	109	9.3	110	8.9
111	7.5	112	6.7	113	3.1	114	4.6	115	8.4	116	8	117	3.5.2	118	5	119	9.3	120	9
121	6.7.5	122	6.7	123	3.1	124	5.1.6	125	6.4	126	8	127	3.2	128	5	129	9.3	130	9
131	7.5	132	7	133	3.1	134	6	135	8.4	136	7.8	137	2	138	5	139	8.9.3	140	9
141	6.5	142	7	143	1	144	3.4.3.2.6	145	4	146	8	147	2	148	3.5	149	4.9.7.4 9.5.8.3	150	9
151	7.5	152	7	153	2.6.4.2.1	154	2.5.3.6	155	8.4	156	8	157	3.2	158	3.5	159	9.3	160	9
161	6.5	162	7	163	2.6.4.6 2.6.1	164	3.2.6	165	8.5.4	166	8	167	3.5.2	168	5	169	9.3	170	9
171	7.5	172	7	173	1	174	3.5.3.2.6	175	4	176	7.8	177	2	178	3.5	179	3	180	9
181	5	182	7	183	2.6.1	184	3.6	185	8.4	186	8	187	2	188	3.5	189	3	190	9
191	5	192	7	193	1	194	2.6	195	7.5.4	196	8	197	2	198	3.5	199	9.3	200	9
201	7.5	202	7	203	3.1	204	6	205	4	206	8	207	2	208	2.5	209	9.3	210	9

211	7.5	212	7	213	2.6.1	214	3.6	215	8.6.4	216	8	217	2	218	5	219	9.3	220	9
221	5	222	7	223	2.6.1	224	2.1.6	225	4	226	8	227	3.2	228	2.5	229	9.3	230	9
231	6.5	232	7	233	1	234	2.2.6	235	7.4	236	8	237	2	238	3.5	239	9.3	240	9
241	7.5	242	7	243	2.6.4.1	244	5.2.6	245	4	246	8	247	3.5.2	248	3.5	249	3	250	9
251	6.5	252	7	253	1	254	3.6	255	8.4	256	8	257	2	258	3.5	259	3	260	9
261	7.5	262	7	263	3.5.1	264	6	265	4	266	8	267	2	268	4.5	269	9.3	270	9
271	5	272	7	273	1	274	6	275	4	276	8	277	2	278	3.5	279	3	280	9
281	5	282	7	283	1	284	3.6	285	4	286	8	287	2	288	5	289	3	290	9
291	7.5	292	7	293	2.3.1	294	6	295	4	296	8	297	2	298	4.5	299	3	300	9
301	7.5	302	7	303	1	304	6	305	4	306	8	307	2	308	5	309	9.3	310	9
311	6.5	312	7	313	2.5.6.1	314	6	315	4	316	8	317	2	318	5	319	9.3	320	9
321	5	322	7	323	1	324	6	325	8.4	326	8	327	2	328	5	329	9.3	330	9
331	5	332	7	333	1	334	5.4.6	335	4	336	8	337	2	338	5	339	3	340	9
341	7.5	342	7	343	2.6.1	344	6	345	4	346	8	347	2	348	5	349	3	350	9
351	7.5	352	7	353	1	354	4.6	355	4	356	8	357	3.5.2	358	5	359	3	360	9
361	6.5	362	5.7	363	1	364	2.6	365	4	366	8	367	2	368	3.5	369	3	370	9
371	5	372	7	373	1	374	6	375	4	376	8	377	2	378	5	379	9.3	380	9
381	7.5	382	7	383	2.1	384	6	385	4	386	8	387	2	388	5	389	3	389	9
391	5	392	7	393	2.4.6.1	394	6	395	4	396	8	397	2	398	5	399	3	400	9
410	5	402	7	403	1	404	4.3.6	405	4	406	8	407	2	408	3.5	409	3	410	9
411	5	412	7	413	1	414	4.2.6	415	4	416	8	417	2	418	5	419	3	420	8.3.9
421	5	422	7	423	2.6.1	424	6	425	8.4	426	8	427	2	428	5	429	3	430	9
431	5	432	7	433	6.1	434	6	435	4	436	8	437	2	438	4.5	439	3	440	9
441	5	442	6.7	443	1	444	6	445	5.8.4	446	8	447	2	448	5	449	3	450	9
451	5	452	7	453	1	454	6	455	7.4	456	8	457	3.5.2	458	4.5	459	9.3	460	9
461	5	462	7	463	1	464	6	465	4	466	8	467	2	468	5	469	3	470	9
471	7.5	472	7	473	1	474	6	475	8.4	476	8	477	2	478	5	479	3	480	9
	3.4.5.6		3.4.5.**6**		**4**.5.6 7.8.9		4.5.6 7.8.**9**		**1**.2.3.4.5		1.2.3.4.**5**		**2**.3.4 5.6.7		2.3.4 5.6.**7**		**3**.4.5 6.7.8		3.4.5 6.7.**8**
1	3	2	6	3	4	4	9	5	1	6	5	7	2	8	7	9	4.3	10	8
481	6.5	482	7	483	2.6.1	484	6	485	4	486	8	487	2	488	5	489	3	490	9
	3.4.5.6		3.4.5.**6**		**4**.5.6 7.8.9		4.5.6 7.8.**9**		**1**.2.3.4.5		1.2.3.4.**5**		**2**.3.4 5.6.7		2.3.4 5.6.**7**		**3**.4.5 6.7.8		3.4.5 6.7.**8**
11	5.3	12	6	13	5.4	14	9	15	2.5.1	16	5	17	5.2	18	7	19	7.3	20	8

TABLE 8

Daily Series and Averages with Ratios of Correct to Incorrect First Choices

Problem 3

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
July							July						
11	1–	5	5	5	5	1:1	11	1–	2	8	2	8	1:4.00
12	11–	0	10				12	11–	4	6			
“	21–	3	7	3	17	1:5.66	“	21–	1	9	5	15	1:3.00
13	31–	1	9				13	31–	2	8			
“	41–	3	7	4	16	1:4.00	“	41–	3	7	5	15	1:3.00
14	51–	4	6				14	51–	1	9			
“	61–	3	7	7	13	1:1.86	“	61–	1	9	2	18	1:9.00
15	71–	4	6				15	71–	2	8			
“	81–	2	8	6	14	1:2.33	“	81–	3	7	5	15	1:3.00
16	91–	2	8				16	91–	4	6			
“	101–	4	6	6	14	1:2.33	“	101–	4	6	8	12	1:1.50
17	111–	3	7				17	111–	3	7			
“	121–	3	7	6	14	1:2.33	“	121–	3	7	6	14	1:2.33
18	131–	5	5				18	131–	5	5			
“	141–	6	4				“	141–	6	4			
“	151–	5	5	16	14	1: .88	“	151–	3	7	14	16	1:1.14
19	161–	4	6				19	161–	4	6			
“	171–	6	4	10	10	1:1	“	171–	6	4	10	10	1:1
20	181–	4	6				20	181–	6	4			
“	191–	3	7	7	13	1:1.86	“	191–	6	4	12	8	1: .67
21	201–	6	4				21	201–	6	4			
“	211–	5	5	11	9	1: .82	“	211–	5	5	11	9	1: .82
22	221–	8	2				22	221–	5	5			
“	231–	7	3	15	5	1: .33	“	231–	5	5	10	10	1:1
23	241–	9	1				23	241–	5	5			
“	251–	8	2	17	3	1: .18	“	251–	6	4	11	9	1: .82
24	261–	9	1				24	261–	6	4			
“	271–	8	2				“	271–	9	1			
“	281–	7	3	24	6	1: .25	“	281–	9	1	24	6	1: .25
25	291–	7	3				25	291–	7	3			
“	301–	7	3				“	301–	8	2			
“	311–	9	1	23	7	1: .30	“	311–	7	3	22	8	1: .36
26	321–	6	4				26	321–	8	2			
“	331–	9	1	15	5	1: .33	“	331–	9	1	17	3	1: .18
27	341–	6	4				27	341–	8	2			
“	351–	9	1	15	5	1: .33	“	351–	7	3	15	5	1: .33
28	361–	9	1				28	361–	6	4			
“	371–	9	1	18	2	1: .11	“	371–	9	1	15	5	1: .33
29	381–	7	3				29	381–	8	2			
“	391–	9	1	16	4	1: .25	“	391–	9	1	17	3	1: .18
30	401–	8	2				30	401–	8	2			
“	411–	10	0	18	2	1: .11	“	411–	8	2	16	4	1: .25
							31	421–	8	2			
							“	431–	8	2	16	4	1: .25
							Aug.						
							1	441–	8	2			
							“	451–	6	4			
							“	461–	10	0	24	6	1: .25
2	421	7	3	7	3	1: .43	2	471	8	2	8	2	1: .25
3	1-10	7	3	7	3	1: .43	3	1-10	9	1	9	1	1: .11
3	431	7	3	7	3	1: .43	3	481	8	2	8	2	1: .25
3	11-20	8	2	8	2	1: .25	3	11-20	5	5	5	5	1:1

Although these figures are far from conclusive, we are convinced from the behavior of the animals that neither was choosing by familiarity with the particular settings. She, as has been pointed out, did as well with the control series as with the regular series, and he did even better in the first control series than in the regular series, while showing extreme confusion in the second control series. This was doubtless due to insufficient hunger and the distracting influence of a mistake in the first trial of the series. His carelessness throughout the last control series was conspicuous.

Comparison of the results for problems 2 and 3 indicate that for the female problem 3 was somewhat the more difficult, whereas for the male, problem 2 required a larger number of trials. We are by no means convinced by this comparison that the problems have not been used in the order of increasing difficultness, for we consider the female subject a much more reliable individual than the male, and we suspect that his greater facility in the solution of the third problem was due in part, at least, to the experience of the experimenters in dealing with his temperamental and other peculiarities.

PROBLEM 4

The data to be considered in this connection appear in tables 9, 10 and 11. The correct mechanism is definable simply as the middle one, and the expectation prior to experience is one correct to four incorrect first choices, since the total number of doors open in the series of ten settings is fifty. As is shown in table 11, precisely this ratio resulted from the first day's experimentation in the case of each individual.

Ten trials per series were given regularly throughout the work on this problem.

Unlike the preceding problems, this one proved insoluble. Consequently, the detailed results as they appear in tables 9 and 10 are especially important, since from them may be read the reactive tendencies and their relations to one another. It is, of course, easy to understand why the ratio of correct to incorrect first choices should change steadily in the direction of the solution of the problem, for each subject gradually learned to react appropriately to certain of the settings while failing to acquire the ability to react to the relation middleness.

TABLE 9

Results for Female in Problem 4

T.	S. 1 2.**3**.4	T.	S. 2 5.6.**7**.8.9	T.	S. 3 1.2.3.**4** 5.6.7	T.	S. 4 7.**8**.9	T.	S. 5 4.5.**6**.7.8	T.	S. 6 1.2.3.4.**5** 6.7.8.9	T.	S. 7 1.**2**.3	T.	S. 8 2.3.**4**.5.6	T.	S. 9 3.4.5.**6** 7.8.9	T.	S.10 6.**7**.8
1	2.3	2	5.9.5.9 6.5.9.9 5.9.8.9 5.9.8.6 7	3	1.7.4	4	7.9.7.8	5	4.8.4.8.6	6	1.9.5	7	1.3.2	8	2.5.6 2.6.3 6.5.4	9	3.9.5 9.3.8 9.3.6	10	6.8.7
11	3.4.3	12	5.8.9 6.5.9 6.8.7	13	1.7.3.6 1.5.3.7 2.1.6.5 2.7.5.6 4	14	9.8	15	6	16	5	17	2	18	6.3.5.6 2.3.6.5 3.6.2.4	19	3.8.9.6	20	7
21	2.4.4.3	22	5.8.7	23	1.6.4	24	7.8	25	6	26	1.8.5	27	1.2	28	5.4	29	5.8.3 9.7.6	30	7
31	2.4.3	32	7	33	3.4	34	9.7.8	35	5.6	36	5	37	3.2	38	3.6.4	39	6	40	8.7
41	2.3	42	5.7	43	6.3.4	44	8	45	6	46	2.7.9.1 4.2.9.5	47	3.2	48	5.4	49	6	50	6.8.7
51	2.4.3	52	7	53	5.1.6.7 5.6.2.4	54	7.8	55	6	56	1.9.7.3.5	57	2	58	6.3.5 7.2.4	59	3.6	60	7
61	2.4.3	62	8.6.5.7	63	5.3.6.1 2.7.5.2 6.1.4	64	8	65	5.6	66	6.7.9 3.2.5	67	2	68	6.3.4	69	3.6	70	6.8.6.8.7
71	4.2.3	72	8.5.9.6.7	73	5.6.2.5 1.7.4	74	9.8	75	5.6	76	5	77	2	78	4	79	7.8.3.7 8.3.5.6	80	7
81	3	82	7	83	2.6.4	84	8	85	5.4.6	86	2.6.4.5	87	2	88	6.3.4	89	7.8.5.6	90	6.7
91	4.2.3	92	6.8.6.7	93	2.6.5.4	94	8	95	5.7.5.6	96	6.7.2.9 2.4.6.5	97	2	98	6.4	99	7.6	100	8.6.8.7
101	4.2.3	102	5.6.7	103	5.4	104	7.8	105	6	106	2.4.6.7.5	107	2	108	6.5.4	109	8.7.6	110	.87
111	2.3	112	8.6.7	113	2.5.4	114	8	115	5.6	116	2.4.5	117	2	118	6.6.5.4	119	6	120	6.7
121	3	122	8.6.7	123	2.5.6.4	124	7.8	125	5.6	126	1.4.3.5	127	2	128	4	129	3.6	130	7
131	3	132	7	133	4	134	8	135	6	136	5	137	2	138	6.5.6.4	139	6	140	8.7
141	4.3	142	7	143	2.5.6.5 6.1.4	144	8	145	7.6	146	6.5	147	2	148	6.4	149	8.9.6	150	8.7
151	3	152	7	153	2.5.6.7 5.2.4	154	8	155	7.6	156	7.8.4 8.8.5	157	2	158	5.3.6.4	159	8.7.3.7 8.4.6	160	6.8.7
161	2.4.3	162	7	163	4	164	8	165	6	166	2.5	167	2	168	3.4	169	8.7.4.6	170	7
171	3	172	5.7	173	2.4	174	8	175	5.6	176	2.3.5	177	2	178	3.4	179	6	180	7
181	3	182	8.7	183	2.4	184	7.8	185	5.7.6	186	2.4.7.8.5	187	2	188	4	189	8.7.6	190	8.7
191	3	192	6.7	193	2.4	194	7.8	195	5.6	196	2.4.8.4.5	197	2	198	6.3.4	199	6	200	7
201	3	202	7	203	5.4	204	7.8	205	5.6	206	2.4.5	207	2	208	4	209	6	210	6.8.7
211	3	212	6.8.5.7	213	5.4	214	7.8	215	6	216	4.6.2.5	217	2	218	6.4	219	8.7.6	220	6.7
221	3	222	6.8.5.7	223	5.6.4	224	8	225	5.6	226	5	227	2	228	6.4	229	8.7.5.6	230	7

231	3	232	7	233	4	234	8	235	6	236	2.1.5	237	2	238	6.5.3.4	239	6	240	7
241	3	242	6.8.7	243	4	244	8	245	5.6	246	2.4.6 8.9.5	247	3.2	248	6.5.4	249	8.9.5.7.6	250	8.7
251	4.3	252	7	253	4	254	8	255	5.6	256	2.4.5	257	3.2	258	3.4	259	4.5.6	260	7
261	3	262	7	263	4	264	8	265	7.6	266	5	267	2	268	5.4	269	6	270	7
271	4.3	272	8.7	273	6.5.7.4	274	8	275	6	276	4.6.2.6 8.6.4.7 4.3.2.4 6.7.5	277	3.2	278	5.4	279	6	280	7
281	4.3	282	7	283	6.4	284	8	285	6	286	2.6.4.5	287	2	288	5.4	289	6	290	7
291	4.3	292	8.6.7	293	5.6.4	294	8	295	6	296	4.6.5	297	3.2	298	6.5.4	299	7.6	300	7
301	3	302	6.7	303	4	304	7.8	305	6	306	7.5	307	3.2	308	6.5.4	309	7.8.7.6	310	8.7
311	3	312	6.5.7	313	5.6.4	314	8	315	5.6	316	4.7.6.2 4.3.5	317	3.2	318	4	319	4.7.6	320	8.6.7
321	4.3	322	6.8.7	323	7.6.2.4	324	8	325	7.5.6	326	2.4.3.5	327	2	328	6.4	329	8.6	330	7
331	3	332	6.7	333	4	334	8	335	4.5.6	336	2.4.5	337	2	338	6.4	339	6	340	7
341	4.3	342	7	343	2.4	344	8	345	4.6	346	2.4.6 7.3.5	347	2	348	6.4	349	8.4.6	350	7
351	3	352	6.6.8.7	353	5.6.4	354	8	355	5.6	356	6.7.5	357	2	358	4	359	8.7.6	360	8.7
361	4.3	362	6.8.5.7	363	4	364	7.8	365	5.6	366	5	367	2	368	6.5.4	369	8.7.5.6	370	7
371	4.3	372	7	373	5.6.4	374	8	375	7.6	376	7.8.6.4.5	377	2	378	6.4	379	7.6	380	7
381	4.3	382	8.7	383	6.5.4	384	8	385	6	386	5	387	2	388	6.5.4	389	7.6	390	7
391	3	392	8.7	393	5.6.4	394	8	395	5 .6	396	6.5	397	2	398	5.4	399	6	400	7
401	3	402	7	403	5.6.4	404	8	405	4.5.6	406	2.4.5	407	2	408	4	409	6	410	7
411	3	412	7	413	2.4	414	8	415	5.6	416	2.4.5	417	2	418	3.4	419	6	420	7
421	3	422	6.7	423	4	424	8	425	6	426	2.5	427	2	428	2.4	429	6	430	7
431	3	432	6.7	433	2.6.5.4	434	8	435	4.5.6	436	2.4.5	437	2	438	3.4	439	6	440	8.7
441	3	442	8.7	443	2.4	444	8	445	6	446	6.6.2 4.6.5	447	2	448	6.4	449	6	450	7
451	4.3	452	8.7	453	2.4	454	8	455	7.6	456	2.4.5	457	3.2	458	6.4	459	8.7.6	460	7
461	3	462	7	463	2.4	464	8	465	6	466	2.5	467	2	468	6.4	469	6	470	7
471	3	472	7	473	3.4	474	8	475	7.6	476	2.4.5	477	2	478	4	479	4.6	480	7
481	3	482	7	483	3.5.4	484	8	485	6	486	4.5	487	2	488	4	489	4.5.4.5.6	490	8.7
491	3	492	7	493	5.4	494	8	495	6	496	2.5	497	2	498	4	499	6	500	8.7
501	3	502	8.7	503	6.5.4	504	8	505	7.5.6	506	2.5	507	2	508	6.4	509	7.6	510	7
511	3	512	8.7	513	2.5.6.4	514	8	515	7.6	516	2.5	517	2	518	5.6.4	519	8.7.6	520	7
521	3	522	6.7	523	4	524	8	525	6	526	2.4.5	527	2	528	6.5.4	529	6	530	8.7
531	3	532	7	533	4	534	8	535	7.6	536	5	537	2	538	6.5.4	539	7.6	540	7
541	3	542	8.7	543	4	.544	8	545	7.6	546	2.5	547	2	548	6.5.4	549	8.6	550	7
551	4.3	552	8.7	553	6.4	554	8	555	7.6	556	7.4.5	557	2	558	5.4	559	6	560	7
561	4.3	562	8.7	563	4	564	8	565	6	566	5	567	2	568	6.5.4	569	7.6	570	7
571	3	572	8.7	573	4	574	8	575	6	576	4.5	577	2	578	4	579	7.6	580	7
581	3	582	7	583	6.5.4	584	8	585	7.6	586	6.5	587	2	588	6.5.4	589	7.6	590	7
591	3	592	6.7	593	4	594	7.7.8	595	5.6	596	2.3.4.5	597	2	598	3.5.4	599	5.6	600	7

TABLE 9—*Continued*

RESULTS FOR FEMALE IN PROBLEM 4

T.	S. 1 2.**3**.4	T.	S. 2 5.6.**7**.8.9	T.	S. 3 3.4.**5**.6.7	T.	S. 4 7.**8**.9	T.	S. 5 4.5.**6**.7.8	T.	S. 6 1.2.**3**.4.5	T.	S. 7 1.**2**.3	T.	S. 8 2.3.**4**.5.6	T.	S. 9 4.**5**.6	T.	S. 10 6.**7**.8
1	3	2	7	3	4.5	4	7.8	5	6	6	2.4.3	7	2	8	4	9	5	10	7
11	3	12	7	13	6.5	14	8	15	6	16	5.4.2 5.4.3	17	2	18	4	19	6.5	20	7
21	3	22	8.7	23	4.5	24	8	25	6	26	2.4.5.2 4.5.4.3	27	2	28	4	29	5	30	7
31	3	32	7	33	7.6.5	34	8	35	7.6	36	2.4.5.4 4.2.4.3	37	2	38	5.4	39	5	40	7
41	3	42	7	43	6.5	44	7.8	45	7.6	46	2.4.3	47	2	48	4	49	5	50	7
51	3	52	6.7	53	4.5	54	8	55	6	56	4.5.4.2.3	57	2	58	5.6.4	59	6.5	60	7
61	3	62	7	63	4.5	64	8	65	6	66	2.4.3	67	2	68	5.4	69	6.5	70	8.7
71	3	72	7	73	4.5	74	7.8	75	6	76	4.2.3	77	2	78	4	79	6.5	80	8.6.7
81	3	82	7	83	4.6.5	84	7.8	85	5.4.6	86	2.4.3	87	2	88	4	89	6.5	90	7
91	3	92	7	93	6.5	94	8	95	4.6	96	2.3	97	2	98	4	99	5	100	7
101	3	102	7	103	6.5	104	8	105	6	106	2.4.5.2.3	107	3.2	108	4	109	6.5	110	8.7
111	3	112	7	113	6.5	114	8	115	6	116	2.4.3	117	2	118	5.4	119	5	120	8.7
121	3	122	8.7	123	5	124	8	125	6	126	4.3	127	2	128	4	129	6.5	130	7
131	3	132	7	133	6.5	134	7.8	135	6	136	4.3	137	2	138	6.5.4	139	6.5	140	7
141	4.3	142	7	143	6.5	144	8	145	6	146	2.4.5.3	147	2	148	5.4	149	6.5	150	7
151	3	152	7	153	5	154	8	155	7.6	156	4.3	157	2	158	5.6.4	159	6.5	160	8.7
161	3	162	7	163	6.5	164	8	165	6	166	4.3	167	3.2	168	5.4	169	5	170	8.7
171	3	172	7	173	5	174	8	175	6	176	2.3	177	2	178	4	179	5	180	7
181	3	182	7	183	6.5	184	8	185	6	186	2.4.3	187	2	188	4	189	4.6.5	190	8.7
191	3	192	6.5.8.7	193	4.3.5	194	8	195	6	196	3	197	2	198	6.3.4	199	5	200	7
201	3	202	8.7	203	6.4.5	204	8	205	6	206	4.3	207	2	208	5.4	209	6.5	210	7

TABLE 10

Results for Male in Problem 4

T.	S. 1 2.**3**.4	T.	S. 2 5.6.**7**.8.9	T.	S. 3 1.2.3.**4** 5.6.7	T.	S. 4 7.**8**.9	T.	S. 5 4.5.**6**.7.8	T.	S. 6 1.2.3.4.**5** 6.7.8.9	T.	S. 7 1.**2**.3	T.	S. 8 2.3.**4**.5.6	T.	S. 9 3.4.5.**6** 7.8.9	T.	S. 10 6.**7**.8
1	2.4.2.4.3	2	5.9.5.9.7	3	1.7.1.7.4	4	7.9.7 9.7.8	5	4.8.7.8 4.8.4.7 4.8.5.7 4.8.5.8 6	6	1.9.2 8.6.5	7	1.3.2	8	2.6.5.4	9	3.9.5.8 3.9.6	10	7
11	2.4.3	12	5.8.6.9.7	13	1.6.5.7 2.3.5 1.7.4	14	8	15	5.7.4.6	16	2.6.3.9 1.4.5	17	2	18	3.2.5.6.4	19	8.3.5.9.6	20	7
21	2.3	22	6.5.8.9.7	23	5.1.4	24	8	25	4.5.7.4.6	26	1.3.2.4 1.9.7.5	27	2	28	6.5.4	29	5.3.8.6	30	6.8.6.7
31	2.4.3	32	5.8.9.6.7	33	5.1.6.4	34	8	35	5.7.6	36	2.8.6 4.2.5	37	3.1.2	38	3.5.6.5.4	39	3.6	40	7
41	2.3	42	6.5.8.9.7	43	4	44	8	45	6	46	2.5	47	1.2	48	3.2.6.5 3.6.5 6.2.4	49	8.3.8.7.6	50	7
51	2.3	52	7	53	1.7.4	54	8	55	5.6	56	2.6.1.4 3.2.5	57	2	58	2.5.4	59	3.6	60	7
61	3	62	8.6.5.7	63	5.2.1.6 5.7.3.4	64	7.8	65	7.4.5.6	66	3.9.5	67	2	68	3.4	69	7.9.8 5.9.8 7.3.6	70	8.7
71	3	72	8.6.5.5.7	73	6.5.2.4	74	8	75	5.4.5.6	76	2.3.7.7 8.2.3.8 4.6.7.2 5	77	3.2	78	6.4	79	8.7.9.6	80	7
81	4.3	82	9.7	83	2.4	84	8	85	6	86	5	87	2	88	6.5.3.4	89	7.3.6	90	8.7
91	2.4.3	92	6.8.6.5.7	93	6.2.4	94	8	95	7.5.4.6	96	5	97	2	98	5.6.4	99	6	100	7
101	2.4.3	102	7	103	1.4	104	7.8	105	5.6	106	7.3.5	107	2	108	4	109	6	110	7
111	3	112	8.6.5.7	113	5.6.2.7.4	114	7.8	115	6	116	7.5	117	2	118	5.6.4	119	5.8.7.6	120	8.7
121	3	122	6.5.8.6.7	123	2.4	124	8	125	5.7.4.7.6	126	2.3.6.1.5	127	2	128	3.4	129	8.6	130	8.7
131	3	132	5.7	133	2.4	134	8	135	6	136	2.6.7.5	137	2	138	6.5.4	139	8.7.6	140	8.7
141	4.2.3	142	7	143	2.5.7.6.4	144	8	145	6	146	6.5	147	2	148	6.4	149	8.7.6	150	8.7
151	2.4.3	152	6.7	153	1.6.4	154	8	155	5.6	156	7.2.5	157	2	158	6.2.6.4	159	6	160	8.7
161	2.4.3	162	8.6.7	163	2.4	164	8	165	6	166	2.4.2.6.5	167	2	168	6.5.4	169	7.6	170	8.7
171	3	172	8.7	173	7.6.5 6.2.4	174	8	175	5.7.5.6	176	5	177	2	178	5.6.5.4	179	6	180	8.7
181	3	182	8.6.5.7	183	5.2.4	184	8	185	6	186	2.5	187	2	188	2.4	189	3.7.5.6	190	8.7
191	3	192	7	193	2.4	194	8	195	5.6	196	6.2.4.5	197	2	198	5.4	199	6	200	7
210	3	202	7	203	2.4	204	8	205	5.6	206	2.5	207	2	208	4	209	8.7.6	210	8.6.7
211	2.4.3	312	6.7	213	2.5.7.4	214	8	215	6	216	2.6.4.3.5	217	2	218	6.4	219	8.7.6	220	8.7
221	3	222	6.5.6.8.7	223	2.4	224	8	225	6	226	2.3.4.5	227	2	228	3.4	229	7.6	230	8.7

RESULTS FOR MALE IN PROBLEM I

T.	S. 1	T.	S. 2	T.	S. 3	T.	S. 4	T.	S. 5	T.	S. 6	T.	S. 7	T.	S. 8	T.	S. 9	T.	S. 10
T.	2.3.4	T.	5.6.7.8.9	T.	1.2.3.4 5.6.7	T.	7.8.9	T.	4.5.6.7.8	T.	1.2.3.4.5 6.7.8.9	T.	1.2.3	T.	2.3.4.5.6	T.	3.4.5.6 7.8.9	T.	6.7.8
231	3	232	8.7	233	5.6.7.4	234	8	235	7.6	236	2.5	237	2	238	6.5.6.4	239	8.7.5.7.6	240	7
241	3	242	8.7	243	2.4	244	8	245	5.6	246	2.4.7 2.4.5	247	2	248	3.5.6.4	249	6	250	7
251	3	252	8.6.5.6.7	253	2.6.3.4	254	8	255	6	256	2.5	257	2	258	3.4	259	3.5.6	260	7
261	3	262	7	263	5.4	264	7.8	265	5.6	266	2.5	267	2	268	5.6.4	269	6	270	7
271	2.4.3	272	6.6.7	273	2.3.5.4	274	8	275	4.6	276	6.7.3.4.5	277	3.2	278	4	279	6	280	8.7
281	3	282	6.5.7	283	2.4	284	8	285	5.4.6	286	2.3.5	287	2	288	4	289	5.7.6	290	8.7
291	3	292	8.7	293	5.6.4	294	8	295	6	296	7.7.2.6.5	297	2	298	5.6.6.4	299	7.6	300	7
301	3	302	8.6.7	303	5.6.4	304	8	305	6	306	2.6.5	307	2	308	5.6.2.6.4	309	6	310	8.6.7
311	3	312	7	313	2.4	314	7.8	315	5.7.6	316	8.5	317	2	318	6.4	319	8.7.6	320	8.7
321	3	322	6.8.7	323	5.6.5 7.6.4	324	8	325	7.5.4.6	326	2.5	327	2	328	6.5.6.4	329	8.6	330	8.7
331	3	332	6.5.8.7	333	2.5.6.4	334	8	335	6	336	2.5	337	2	338	5.4	339	6	340	7
341	2.4.3	342	8.7	343	2.5.4	344	8	345	5.6	346	2.5	347	2	348	5.6.4	349	7.6	350	8.7
351	3	352	8.7	353	5.4	354	7.8	355	5.6	356	5	357	2	358	5.6.5.4	359	8.7.6	360	8.7
361	3	362	8.6.7	363	2.5.6.4	364	8	365	6	366	2.4.6.5	367	3.2	368	6.4	369	6	370	7
371	3	372	7	373	1.4	374	8	375	5.6	376	2.5	377	2	378	4	379	6	380	7
381	3	382	6.5.7	383	6.5.7.6.4	384	7.8	385	6	386	2.7.6 7.6.5	387	2	388	6.4	389	6	390	7
391	2.4.3	392	6.5.7	393	6.4	394	8	395	5.6	396	2.4.7.5	397	2	398	6.4	399	6	400	7
401	2.3	402	7	403	2.6.4	404	7.8	405	5.4.6	406	5	407	2	408	5.4	409	7.6	410	8.7
411	3	412	7	413	4	414	8	415	5.6	416	5	417	3.2	418	3.4	419	5.6	420	7
421	3	422	7	423	2.4	424	8	425	5.6	426	2.4.5	427	2	428	4	429	7.3.5.6	430	7
431	3	432	7	433	6.4	434	7.8	435	6	436	2.3.4 2.3.1 6.4.5	437	2	438	3.4	439	8.7.6	440	7
441	2.4.3	442	6.5.7	443	4	444	7.8	445	6	446	2.3.5	447	2	448	2.6.4	449	3.4.6	450	7
451	2.4.3	452	7	453	2.6.5.4	454	7.8	455	6	456	2.6.4.5	457	2	458	6.4	459	7.6	460	7
461	3	462	7	463	5.4	464	8	465	6	466	5	467	2	468	6.4	469	7.6	470	7
471	3	472	8.7	473	1.4	474	8	475	6	476	1.7.5	477	2	478	6.4	479	7.6	480	7
481	3	482	8.7	483	5.3.2.6.4	484	8	485	6	486	7.4.5	487	2	488	4	489	6	490	7
491	3	492	6.5.7	493	6.7.4	494	8	495	7.5.6	496	7.5	497	2	498	6.6.6 5.6.4	499	6	500	7
501	3	502	8.6.6.5.7	503	2.4	504	8	505	7.6	506	6.5	507	2	508	6.4	509	7.6	510	7
511	3	512	7	513	4	514	8	515	6	516	7.6.5	517	2	518	2.4	519	5.7.8.6	520	7
521	3	522	7	523	5.4	524	8	525	7.6	526	5	527	2	528	5.6.4	529	6	530	8.7
531	3	532	7	533	6.4	534	8	535	6	536	5	537	2	538	6.4	539	7.6	540	8.7
541	3	542	8.6.7	543	5.6.4	544	8	545	6	546	5	547	2	548	6.4	549	6	550	7
551	3	552	7	553	2.5.6.4	554	8	555	6	556	4.5	557	3.2	558	4	559	7.6	560	8.7
561	3	562	7	563	6.5.4	564	8	565	6	566	2.5	567	2	568	4	569	5.4.6	570	7
571	3	572	7	573	5.6.4	574	8	575	6	576	5	577	2	578	6.4	579	6	580	7
581	3	582	8.6.5.7	583	5.4	584	8	585	5.6	586	4.5	587	2	588	4	589	5.4.6	590	6.7
591	3	592	7	593	6.4	594	8	595	5.6	596	5	597	2	598	4	599	8.6	600	7

TABLE 10—*Continued*

RESULTS FOR MALE IN PROBLEM 4

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	2.**3**.4	T.	5.6.**7**.8.9	T.	3.4.**5**.6.7	T.	7.**8**.9	T.	4.5.**6**.7.8	T.	1.2.**3**.4.5	T.	1.**2**.3	T.	2.3.**4**.5.6	T.	4.**5**.6	T.	6.**7**.8
1	3	2	8.7	3	6.4.5	4	7.8	5	6	6	2.4.2.5.3	7	2	8	5.4	9	6.5	10	7
11	3	12	7	13	6.5	14	8	15	6	16	2.4.5 2.4.3	17	2	18	4	19	6.6.5	20	8.7
21	3	22	8.7	23	6.5	24	8	25	6	26	2.4.5.3	27	2	28	4	29	6.5	30	7
31	3	32	7	33	6.4.5	34	7.8	35	6	36	2.4.5.2.3	37	2	38	6.4	39	6.5	40	7
41	3	42	8.7	43	6.5	44	8	45	6	46	4.4.5 2.4.3	47	2	48	4	49	4.5	50	7
51	3	52	6.7	53	6.4.5	54	8	55	6	56	4.5.3	57	2	58	4	59	5	60	6.7
61	3	62	7	63	4.5	64	7.8	65	6	66	4.3	67	2	68	4	69	5	70	7
71	3	72	7	73	4.5	74	8	75	4.5.6	76	2.4.3	77	2	78	4	79	4.5	80	6.7
81	2.4.3	82	6.5.6.7	83	4.5	84	7.8	85	6	86	2.4.3	87	2	88	4	89	6.5	90	7
91	2.4.3	92	7	93	4.5	94	7.8	95	4.5.6	96	4.3	97	2	98	4	99	4.5	100	7
101	2.4.3	102	6.5.7	103	5	104	8	105	7.6	106	4.5.3	107	2	108	4	109	5	110	8.7
111	3	112	7	113	5	114	8	115	6	116	2.3	117	2	118	6.4	119	5	120	8.7
121	3	122	8.7	123	6.4.5	124	8	125	6	126	4.2.3	127	2	128	5.6.4	129	6.5	130	7
131	3	132	7	133	5	134	8	135	6	136	4.2.3	137	2	138	3.4	139	5	140	7
141	3	142	7	143	5	144	8	145	6	146	4.2.3	147	2	148	4	149	5	150	7
151	3	152	8.7	153	4.5	154	8	155	4.5.6	156	4.5.3	157	2	158	4	159	5	160	7
161	3	162	7	163	5	164	8	165	6	166	2.4.3	167	2	168	4	169	5	170	7
171	3	172	7	173	5	174	8	175	6	176	2.4.3	177	2	178	4	179	5	180	7
181	3	182	7	183	6.4.5	184	8	185	6	186	2.3	187	2	188	4	189	6.5	190	7
191	3	192	7	193	6.5	194	7.8	195	5.6	196	3	197	2	198	6.5.6.4.3	199	5	200	7
201	3	202	7	203	5	204	8	205	6	206	2.3	207	2	208	4	209	6.5	210	7

TABLE 11

Daily Series and Averages with Ratios of Correct to Incorrect First Choices

Problem 4

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug.							Aug.						
4	1–	0	5				4	1	0	5			
"	6–	0	5				"	6–	1	4			
"	11–	4	6	4	16	1:4.00	"	11–	3	7	4	16	1:4.00
5	21–	2	8				5	21–	2	8			
"	31–	3	7	5	15	1:3.00	"	31–	2	8	4	16	1:4.00
6	41–	3	7				6	41–	4	6			
"	51–	4	6	7	13	1:1.86	"	51–	4	6	8	12	1:1.50
7	61–	2	8				7	61–	2	8			
"	71–	4	6	6	14	1:2.33	"	71–	3	7	5	15	1:3.00
8	81–	4	6				8	81–	4	6			
"	91–	2	8				"	91–	5	5			
"	101–	2	8	8	22	1:2.75	"	101–	5	5	14	16	1:1.14
9	111–	3	7				9	111–	3	7			
"	121–	4	6				"	121–	3	7			
"	131–	8	2	15	15	1:1	"	131–	4	6	10	20	1:2.00
10	141–	3	7				10	141–	4	6			
"	151–	4	6				"	151–	3	7			
"	161–	6	4	13	17	1:1.31	"	161–	3	7	10	20	1:2.00
11	171–	5	5				11	171–	5	5			
"	181–	3	7				"	181–	4	6			
"	191–	4	6	12	18	1:1.50	"	191–	6	4	15	15	1:1
12	201–	5	5				12	201–	5	5			
"	211–	3	7				"	211–	3	7			
"	221–	5	5	13	17	1:1.31	"	221–	4	6	12	18	1:1.50
13	231–	8	2				13	231–	4	6			
"	241–	3	7				"	241–	5	5			
"	251–	4	6	15	15	1:1	"	251–	5	5	14	16	1:1.14
14	261–	8	2				14	261–	5	5			
"	271–	4	6				"	271–	3	7			
"	281–	6	4	18	12	1: .67	"	281–	4	6	12	18	1:1.50
15	291–	3	7				15	291–	5	5			
"	301–	3	7				"	301–	5	5			
"	311–	3	7	9	21	1:2.33	"	311–	3	7	13	17	1:1.31
16	321–	3	7				16	321–	3	7			
"	331–	6	4				"	331–	6	4			
"	341–	4	6	13	17	1:1.31	"	341–	2	8	11	19	1:1.73
17	351–	4	6				17	351–	3	7			
"	361–	4	6				"	361–	5	5			
"	371–	4	6	12	18	1:1.50	"	371–	7	3	15	15	1:1
18	381–	5	5				18	381–	5	5			
"	391–	5	5				"	391–	4	6			
"	401–	7	3	17	13	1: .76	"	401–	3	7	12	18	1:1.50
19	411–	6	4				19	411–	6	4			

TABLE 11—*Continued*

Daily Series and Averages with Ratios of Correct to Incorrect First Choices

Problem 4

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug.							Aug.						
"	421–	7	3				"	421–	6	4			
"	431–	4	6	17	13	1: .76	"	431–	5	5	17	13	1: .76
20	441–	6	4				20	441–	4	6			
"	451–	2	8				"	451–	4	6			
"	461–	7	3	15	15	1:1	"	461–	7	3	15	15	1:1
21	471–	6	4				21	471–	5	5			
"	481–	6	4	12	8	1: .67	"	481–	7	3	12	8	1: .67
22	491–	7	3				22	491–	5	5			
"	501–	4	6				"	501–	4	6			
"	511–	4	6	15	15	1:1	"	511–	7	3	16	14	1: .88
23	521–	6	4				23	521–	6	4			
"	531–	7	3				"	531–	6	4			
"	541–	5	5	18	12	1: .67	"	541–	7	3	19	11	1: .58
24	551–	4	6				24	551–	5	5			
"	561–	6	4				"	561–	7	3			
"	571–	7	3	17	13	1: .76	"	571–	8	2	20	10	1: .50
25	581–	5	5				25	581–	4	6			
"	591–	4	6	9	11	1:1.22	"	591–	7	3	11	9	1: .82
25	1–	7	3	7	3	1: .43	25	1–	4	6	4	6	1:1.50
26	11–	7	3				26	11–	6	4			
"	21–	7	3				"	21–	6	4			
"	31–	6	4	20	10	1: .50	"	31–	5	5	17	13	1: .76
27	41–	6	4				27	41–	6	4			
"	51–	5	5				"	51–	6	4			
"	61–	5	5	16	14	1: .88	"	61–	7	3	19	11	1:.58
28	71–	5	5	5	5	1:1	28	71–	5	5	5	5	1:1
30	81–	5	5				30	81–	4	6			
"	91–	7	3				"	91–	4	6			
"	101–	5	5	17	13	1: .76	"	101–	5	5	13	17	1:1.31
31	111–	6	4				31	111–	7	3			
"	121–	7	3				"	121–	5	5			
"	131–	5	5	18	12	1: .67	"	131–	8	2	20	10	1: .50
Sept.							Sept.						
1	141	5–	5				1	141–	9	1			
"	151	5–	5				"	151–	6	4			
"	161	5–	5	15	15	1:1	"	161–	9	1	24	6	1: .25
2	171	9–	1				2	171–	9	1			
"	181	6–	4				"	181–	7	3			
"	191	7–	3	22	8	1: .36	"	191–	6	4	22	8	1: .36
3	201	5–	5	5	5	1:1	3	201–	8	2	8	2	1: .25

After six hundred trials had been given to each individual by use of the series of settings presented on page 192, under problem 4, it was apparent that the animals could succeed in solving the problem only by acquiring a definite habit for each particular setting, and it was further evident that the settings including seven and nine open doors were extremely difficult for the animals. For these reasons it was decided to present a modified series of settings in which the groups should consist of either three or five open doors. Two hundred trials were given with the new series of settings, and the settings themselves, as well as the results obtained, appear at the bottom of tables 9 and 10.

Two important conclusions are justified by these results. First, that the pigs, in so far as they had succeeded in responding correctly to the middle door, had reacted to particular settings. And second, that with sufficiently prolonged training they could perfectly solve the problem of the middle member of a series, if the total number in a group of open doors did not exceed five. As a matter of fact, no series of ten correct choices was obtained with either individual because of the surprisingly strong and persistent influence of the original settings.

Let us consider, for example, setting 3. This originally consisted of the group 1.2.3.**4**.5.6.7, in which no. 4 was the box to be entered. In the modified settings, this group was changed to 3.4.**5**.6.7, consequently, the box to be entered was 5 instead of 4. Now, whereas in the case of setting 1 which remained unchanged, the female made only one mistake in twenty-one trials subsequent to the modification of the settings, in the case of setting 3 she chose wrongly in all except three of the twenty-one trials, and this in spite of the fact that in the case of settings 2 and 5, both of which involved five open doors, she chose correctly sixteen times out of twenty-one. Similarly in the case of setting 6, in which originally all nine of the doors were open, whereas in the modification only doors 1, 2, 3, 4 and 5 were used, both the female and the male chose correctly only once in twenty-one trials.

Although the above conclusions are of primary importance, further examination of the data of tables 9 and 10 should throw additional light on the reactive capacity of our subjects.

We shall consider the materials according to the number of mechanisms used in the settings. Settings 1, 4, 7 and 10 involve three members, setting 2, 5 and 8, five members; settings 3 and 9, seven members; and setting 6, nine members. Below are presented the number of correct first choices made by each individual in connection with each setting, the total number of choices being sixty.

Correct First Choices *in Sixty* for Each Setting in Problem 4

	S.1	S.2	S.3	S.4	S.5	S.6	S.7	S.8	S.9	S.10
Female..................	35	22	16	45	22	10	49	11	22	38
Male.....................	41	21	4	48	27	12	52	11	19	34

These figures prove that to select the middle member of a group of three is fairly easy for the pig. This, to be sure, might be gathered from the fact that the animal can solve the problem of the second from the left. It further appears that attempts to locate the proper box when it was the middle of a series of five resulted in a gradual reduction in the number of incorrect choices, but never yielded success. The selection of the middle member of a group of seven or of nine is clearly still more difficult, and there is no reason to suppose that with less than thousands of trials the subjects in question would have learned to enter it directly.

It is practically certain that the series of settings rather than the number of members in a group is responsible for the animal's confusion. Doubtless by training a pig to react correctly to each setting and by then presenting the several settings in a certain definite order, a habit could be built up which would apparently yield a perfect solution of problem 4. It is, however, needless to point out that this would not be the kind of solution that has been obtained for problems 1, 2 and 3, or in other words, would not be dependent upon response to the general relation middleness.

Analysis of the records for the sixty trials under setting 6 are of special interest, since this setting proved the most baffling of all to the subjects.

To begin with, they naturally tried the end members of the series. This proving unsatisfactory, they next tended to choose rather at random, and then there gradually appeared a tendency to enter, first, box 2 and to proceed thence either directly or by way of 3, 4 and sometimes also 6, to the middle box, number 5. This tendency to select, when in doubt, a box second from the right end of the series may possibly be due in part to the fact that the box to be chosen in setting 7 was number 2. At any rate, the frequency with which the female throughout her training chose box 2 first of all under setting 6 is surprisingly high, whereas for the male, this frequency while rather high early in the course of the training, tended to diminish and to give place to the decidedly profitable tendency to choose a box near the middle of the series, 6, 7 and 5 frequently being entered.

Similarly, we might, if space permitted, analyse in detail the results for the other settings. We have chosen to use our space in this report for the presentation of data in tabular form rather than for their description, because we are convinced that the facts are more important than early attempts at interpretation.

SUMMARY

1. The pig has proved itself an ideal subject for studies in adaptive behavior.

2. The new multiple choice method, by means of which standardized problems ranging in difficultness from the very easy to the very difficult may be presented to widely differing types of organism, has in our opinion fully justified our expectations, for it has proved admirably suited to the discovery and analysis of increasingly complex types of behavior.

3. For the purpose of discovering the extent to which ideational and closely allied types of behavior exist in the pig, four problems were presented. They may be defined simply in terms of the constant relation of the right mechanism, as (1) the first at the right end of the series; (2) the second from the left end of the series; (3) alternately, the first at the left and the first at the right; (4) the middle member of the series.

The purpose of the experiments was to discover the pig's reactive tendencies and especially its degree of ability to dissociate the essential and constant relation of the right mechanism from its accidental and variable accompaniments.

4. The two subjects solved perfectly the first problem with less than fifty experiences. The indications are that visual and kinaesthetic guidance sufficed.

The second problem was solved more slowly, partly because the influence of the earlier training had to be overcome, but also because this is a much more difficult problem than the first one. In this also, visual and kinaesthetic guidance seems to account for success, but the extent to which the animals learned to respond to the relation of secondness from the left, no matter what the other relations of the mechanism, was a surprise to the experimenters and is important in connection with the problem of ideation in animals.

The third problem also was solved with reasonable ease, and the animals demonstrated their ability to acquire the habit of alternation without respect to particular groups of reaction-mechanisms.

Problem 4 proved too difficult for the pigs. They learned to select the middle mechanism of the series when the groups were small, but when seven or nine mechanisms were in use, they were confused. The indications are that with long training they would learn to react to the particular settings correctly, although incapable of reacting to the constant relation of middleness.

5. Our results indicate for the pig an approach to free ideas which we had not anticipated. There seems no reason to doubt that visual and kinaesthetic factors in the main determine their responses, but it is evident that they are not so dependent upon the particular situation as are many other mammals. While hesitating to claim that we have demonstrated the presence of ideas, we are convinced that the pig closely approaches, if he does not actually attain, to simple ideational behavior.

6. The multiple choice method has revealed a number of interesting reactive tendencies, their relations to one another, and the varied ways in which they are manifested in connection with situations which are rather difficult to meet.

7. Finally, we would again call attention to the fact that this method of studying behavior should enable us, when it has been reasonably perfected and its problems standardized, to determine the level of mental development in different individuals, species, stages of growth, and conditions of normality, and to compare the reactive tendencies, whether or not ideational, of other organisms with those of the human subject. Our results thus far fully convince us that the method may be made to yield more valuable psychological and behavioristic information than has any previous approach to ideational problems.

HABIT FORMATION IN THE FIDDLER CRAB

BY BENJAMIN SCHWARTZ AND S. R. SAFIR
From The Biological Laboratory, Cold Spring Harbor

Albrecht Bethe (1898) was the first one to investigate associative memory in crustacea. After analyzing the normal behavior of the green crab, *Carcinus moenas*, on the basis of the structure of the nervous system, Bethe endeavored to discover whether the animal could modify its behavior and thus profit by experience. He liberated a crab in an aquarium containing a cephalopod, *Eledone*, in the darkest corner. The crab, following its instinct to hide, ran to that corner and was immediately seized by the squid. At this point the experimenter interfered, and quickly freeing the animal from its captor, placed it again in the lighted portion of the aquarium. The animal ran back to the dark corner and was again seized by the squid. This experiment was repeated five times with one individual and six times with another, without any evidence that the animal learned to avoid the dangerous corner. Bethe then tried another experiment. He baited a crab with a piece of meat and maltreated it every time that it snapped at the bait. This was repeated a number of times, and as in the previous experiment, he found no modification in the animal's reaction to the stimulus. He, therefore, concluded that the activities of Carcinus are limited to reflexes and instincts, the animal being incapable of exercising any higher mental faculties.

Yerkes (1902) objects to Bethe's conclusion on two grounds: (1) He maintains that the data is altogether insufficient to warrant any generalization, and (2) that the experiments are of such nature that negative results based on more sufficient data would still be inconclusive proof of the animal's inability to profit by experience. It is evident that Bethe endeavored to suppress two fundamental instincts, namely, fear and hunger. To expect an animal to modify these after five or six experiences is almost preposterous. Yerkes therefore tested the American form, *Carcinus granulatus*, for habit formation. He constructed a simple labyrinth containing two blind alleys and one opening. In this labyrinth a number of crabs were liberated daily for a period of two weeks, and were given on an average of four trials per day. It was found that the animals gradually learned to avoid the blind alleys, although even fifty experiences in the case of most did not result in a perfect habit. He also found that if the aquarium was divided into two compartments by means of a wire screen, which contained an opening in its center, the animals learned with increasing rapidity to find the opening, in order to get to the food at the opposite side. From these results Yerkes concluded that Carcinus possesses associative memory.

Yerkes and Huggins (1903) studied habit formation in the crawfish, *Cambarus affinis*. They constructed a labyrinth containing a triangular chamber at one end, while the opposite end contained one closed and one open corner, the latter leading into an aquarium. The animal was placed in the triangular chamber, and could go to either corner in seeking to escape. For one month, each of three animals was given on an average two trials per day. The records of the movements to the closed and open corners showed an increase from fifty to ninety per cent in the direction which led to escape. A test of habit retention after a rest of two weeks, showed that the association persisted. Their general conclusions are: (1) Crawfish are able to learn a simple labyrinth habit, (2) they profit slowly, fifty to a hundred experiences being necessary for perfect association, (3) the chief factors in the habit forming process are smell, touch, sight, and muscular activity, (4) if the possibility of scent is excluded by washing the box after each trial, the animals are still capable of learning.

Other investigators working with different forms have confirmed Yerkes' conclusions. Spaulding (1904) found that the hermit crab, *Eupagurus longicarpus*, which is positively phototactic, could learn to go to a shaded portion of an aquarium for food. The association became so perfect, that the mere introduction of the screen, in order to divide the aquarium into two compartments, caused the animals to run to the shaded half. Drzewina (1908) observed that *Pachygraspus marmoratus*, which is negatively phototactic during the day, reacts positively to light at night. Taking advantage of this tropism, she succeeded in making the crabs come from the shaded side of the box to the side which was artificially illuminated, although in doing this, the animals had to find an opening in the partition which divided the box. The rapidity with which the opening was found increased with successive trials. The same writer* (1910) studied habit formation in the hermit crab, *Clibanarius misanthropus.* She placed tightly corked gastropod shells near naked crabs. The latter immediately fastened themselves upon the shells, trying to pull out the corks. Since all their efforts to enter the shells were in vain, the animals were observed to relax, and at the end of from six to eight days, they became entirely indifferent to their presence. If at this point of the experiment, shells, similarly sealed but of different shape, were introduced, the crabs began to attack them immediately. These results indicate that *Clibanarius* not only possesses associative memory but that it is also able to discriminate form. Cowles (1908) found that *Ocypoda arenaria* could learn to escape from a labyrinth, although it did not learn the position of the exit very accurately. He also found that if he buried a dish of salt water in the sand of their trap, so that the rim of the dish was on a level with the surface of the sand, the crabs learned to climb into the vessel to moisten their gills.

The experiments described hereafter were performed with the fiddler crabs which inhabit the sand spit at Cold Spring Harbor, Long Island. They live on sandy beaches as well as on mud flats, where they construct burrows about one foot in depth. They are diurnal in their habits, and on bright, sunny days they may be seen in large numbers, running hither and thither, feeding and burrowing. The males are particularly striking because of the large cheliped with which they perform curious antics, and which they use as a weapon for combat. When the tide comes in the crabs retreat to their burrows, where they remain until the area above them is again exposed by the receding waters. Their general activities are therefore interrupted at regular intervals, during which they remain perfectly quiet. Their behavior appears to be regular and unchanging, almost stereotyped.

There are two species of fiddler crabs on the sand spit, *Uca*

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*This paper was not available to us. We read an abstract of it in *The Jour. of An. Beh.*, Vol· I, No. 6, pp 450-451, 1911.

pugnax and *Uca pugilator*, the most distinguishing characteristic of the latter being a ridge across the palm of the large cheliped. In the experiments the pugilators were utilized almost exclusively, because of their greater vigor and resistance. The pugnax forms did not thrive in captivity, and became very sluggish. The work with them could not progress very rapidly.

The aim of the experiments was to determine (1) whether the fiddler crabs can form a simple labyrinth habit, (2) whether the habit is retained for a few days, and (3) whether the habit can be broken up.

When the crabs are placed in a wooden box which is about one-half full of moist sand or mud, they immediately begin to seek a means of escape. They usually run to the side opposite which the experimenter is standing, and climb up the sides, near the corners, by inserting the sharply pointed ends of their ambulatory feet into the rough surfaces of the wood. They climb gradually and do not seem to become discouraged by failures. As soon as they reach the top of the box, they escape. It was observed that the animals showed a decided tendency to go to a particular corner, even though escape was rendered impossible by inserting glass plates against the sides. Table 1 gives the records of twelve individuals, showing the corner to which they went, as well as the average interval between two successive trials.

TABLE 1

Individual	No. of trials	Time	Average interval	Corner			
				1	2	3	4
1	25	30 min.	1 min. 12 sec.	1	21	0	3
2	25	44 “	1 “ 47 “	3	19	1	2
3	20	27 “	1 “ 22 “	4	15	0	1
4	20	30 “	1 “ 30 “	0	9	0	11
5	20	28 “	1 “ 24 “	14	6	0	0
6	20	37 “	1 “ 49 “	15	1	4	0
7	20	54 “	2 “ 42 “	11	8	0	1
8	20	24 “	1 “ 12 “	2	18	0	0
9	20	53 “	2 “ 40 “	4	12	3	1
10	30	30 “	1 “ 0 “	3	4	22	1
11	18	31 “	1 “ 43 “	0	5	13	0
12	35	45 “	1 “ 17 “	30	10	3	2

An examination of the above table shows (1) that the interval between two successive trials varies from about one and a half to two minutes, (2) that the animal's desire to liberate itself from the trap persists even when thirty-five trials are given,

(3) that each animal chooses one particular corner from which to escape. The last fact is of the utmost importance, because the labyrinth habit with the fiddler crab involves not only a process of learning, but also the overcoming of a strong inclination. By closing the corner to which the animal is inclined to go, the experimenter is in a position to determine whether the animal can modify its behavior.

TABLE 2

RIGHT HANDED MALES			LEFT HANDED MALES			FEMALES			
No.	Right	Left	No.	Right	Left	No.	Right	Left	Center
1	7	3	1	1	9	1	3	3	4
2	5	5	2	1	9	2	4	5	1
3	7	3	3	2	8	3	4	2	4
4	10	0	4	10	0	4	1	3	6
5	0	10	5	0	10	5	2	1	7
6	7	3	6	0	10	6	2	3	5
7	7	3	7	6	4	7	3	2	5
8	0	10	8	7	3	8	3	3	4
9	1	9	9	3	7	9	2	3	5
10	0	10	10	6	4	10	4	0	6
11	7	3	11	8	2	11	5	2	3
12	7	3	12	2	8	12	1	6	3
13	6	4	13	5	5	13	7	1	2
14	7	3	14	5	5	14	5	3	2
15	9	1	15	5	5	15	1	8	1
16	5	5	16	5	5	16	1	7	2
17	7	3	17	3	7	17	3	5	2
18	5	5	18	2	8	18	4	3	3
19	6	4	19	2	8	19	3	3	4
20	6	4	20	2	8	20	2	2	6
21	7	3	21	0	10	21	4	0	6
22	7	3	22	2	8	22	1	2	7
23	8	2	23	0	10	23	5	2	3
24	5	5	24	2	8	24	6	2	2
25	4	6	25	2	8	25	5	3	2
26	9	1	26	3	7	26	2	5	3
27	9	1	27	8	2	27	1	8	1
28	7	3	28	7	3	28	6	1	3
29	10	0	29	2	8	29	4	2	4
30	9	1	30	4	6	30	5	2	3
31	9	1	31	1	9	31	1	7	2
32	10	0	32	2	8	32	3	3	4
33	8	2	33	8	2	33	3	5	2
34	7	3	34	4	6	34	5	2	3
35	8	2	35	2	8	35	3	4	3
	226	124		122	228		114	113	123

The reasons for the individual preferences were by no means evident. Since the animals exhibit a positive phototaxis it was at first supposed that this peculiar reaction was caused by light, but it was found that when the latter was eliminated, the preference was not changed. Hence some other explanation had to be sought. An examination of the male crab shows it to be unsymmetrical, owing to the possession of the large cheliped, which may be either on the right or the left side of the body. This suggested the hypothesis that the right handed males are inclined to go to the right side, and the left handed males to the left side. Since the females possess no large cheliped, they were expected to be neutral in this respect. That these expectations were fairly borne out may be seen in table 2.

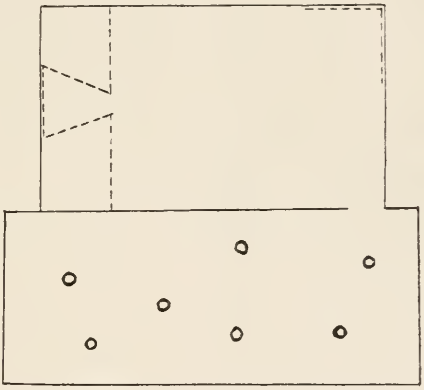

FIG. 1. Showing labyrinth and adjoining box. Dotted lines represent glass plates, and circles represent burrows.

An examination of the data presented above shows that 70% of the males made a majority of their movements to the corner corresponding to the position of their cheliped. About 10% showed no preference for either side, while the remainder, about 20%, went to the side directly opposite to the expectation. But the fact remains that about 90% are inclined to go to a particular side. The number of trials for the right handed males is 226 to the right and 124 to the left, giving a ratio of about 2:1, that of the left handed males is 228 to the left and 122 to the right, giving a similar ratio. The females made 114 attempts to the right, 113 to the left, and 123 to the center, giving a ratio of 1:1:1. By an attempt to the center is meant a direct movement from one end of the box to the center of the opposite end, where the animal pauses for about five seconds before going to either corner.

With these facts before us, we subjected ten animals, seven males and three females, picked up at random, to a simple labyrinth test. Our labyrinth was modelled after the one described by Yerkes and Huggins, with modifications adapted to the needs of the fiddler crabs. We selected a box 50 cm. long, 30 cm. wide, and 30 cm. deep. It was filled to a depth of 12 cm. with moist sand, in order to give the animals a natural substratum. By means of glass plates, 12 cm. square, we made a triangular space at the center of one of the narrower ends, with an opening sufficiently large to enable the crabs to pass through. At the corners of the opposite end we cut out openings, 8 cm. wide and 5 cm. high, which could be closed by means of glass plates. To prevent the animals from going to the corners of that end of the box which contained the triangular chamber, we placed glass plates extending at right angles from each side of the opening of the chamber to the long side of the box. Adjoining the labyrinth there was another box, 60 cm. long, 25 cm. wide and 25 cm. deep. It was filled with moist sand to a depth of 12 cm, several artificial burrows being made in the sand. By means of an opening at each end of the longer side, the box could be made to communicate with the labyrinth on either side.

At first both corners of the labyrinth were closed by means of glass plates and each of the animals to be tested was given a series of preliminary trials to determine to which side of the box it was inclined to go. As soon as this was determined, the favorite corner was left closed, while the one opposite was opened. The crab was then placed in the chamber by the experimenter, who took up his position three feet behind the box, remaining perfectly quiet. His position afforded him a good view of the animal's movements, which were carefully noted. As soon as the crab was liberated it made a number of efforts to climb up the sides of the glass chamber. In doing this it fell after each attempt, the shock evidently frightening it. It then abandoned climbing, and either ran out of the enclosure very rapidly, or moved out rather cautiously, going to its favorite corner. Here, too, it made attempts to climb, but making no headway against the smooth surface of the glass, it began to move to the opposite corner. In the beginning the animal would go half way and turn back, trying to climb again. Finally it would venture all the way across, run out through the opening into the adjoining box, and enter one of the burrows. If an individual showed too much stubborness, by remaining at the closed corner even after it had given up its climbing, the experimenter gently drove it in the direction of the open corner. The crab was allowed to remain in this burrow for about a minute, after which it was taken out and again placed in the triangular chamber, the experimenter taking up the same position as before. It usually took the animal about one minute to recover its composure before making a second trial. With successive trials it seemed to learn that there was one corner which afforded an exit, for no sooner than it reached the closed side, it reversed its direction, and liberated itself from the trap. Sometimes a crab would start off in the direction of the closed corner, but before reaching it, would turn and go to the open. Occasionally an animal ran directly to the center and remained there for a few seconds, often running first in one direction and then in another, before making its final choice. Gradually movements to the open corner became more frequent, attaining almost perfection at the end of ten days, with an average of twenty trials per day.

Following, in table 3, are the records of ten* individuals:

TABLE 3

No. 1. *U. pugilator*, female

Day	Closed	Open	% Closed	% Open
1	18	2	90	10
2	15	5	75	25
3	12	8	60	40
4	10	10	50	50
5	10	10	50	50
6	8	12	40	60
7	9	11	45	55
8	7	13	35	65
9	6	14	30	70
10	1	19	5	95

No. 2. *U. pugilator*, male, left handed

Day	Closed	Open	% Closed	% Open
1	18	2	90	10
2	16	4	80	20
3	15	5	75	25
4	18	2	90	10
5	14	6	70	30
6	10	10	50	50
7	3	17	15	85
8	10	10	50	50
9	3	17	15	85
10	3	17	15	85

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* Nos 1, 2, 3, 4 and 10 were experimented with at Cold Spring Harbor. The remaining five were tried at Hunter's Island, New York City.

No. 3. *U. pugilator,* male, right handed

Day	Closed	Open	% Closed	% Open
1	14	6	70	30
2	5	15	25	75
3	4	16	20	80
4	3	17	15	85
5	4	16	20	80
6	1	19	5	95
7	1	19	5	95
8	0	20	0	100
9	1	19	5	95
10	0	20	0	100

No. 4. *U. pugnax,* right handed

Day	Closed	Open	% Closed	% Open
1	13	7	65	35
2	8	12	40	60
3	9	11	45	55
4	2	18	10	90
5	4	16	20	80
6	3	17	15	85
7	4	16	20	80
8	4	16	20	80
9	2	18	10	90
10	3	17	15	85

No. 5. *U. pugilator,* left handed

Day	Closed	Open	% Closed	% Open
1	18	2	90	10
2	15	5	75	25
3	10	10	50	50
4	7	13	35	65
5	9	11	45	55
6	10	10	50	50
7	5	15	25	75
8	4	16	20	80
9	1	19	5	95
10	4	16	20	80

No. 6.* *U. pugilator,* left handed

Day	Closed	Open	% Closed	% Open
1	15	5	75	25
2	8	12	40	60
3	12	8	60	40
4	10	10	50	50
5	10	10	50	50
6	11	9	55	45
7	9	11	45	55

* This animal died at the end of seven days.

No. 7.* *U. pugilator,* left handed

Day	Closed	Open	% Closed	% Open
1	15	5	75	25
2	9	11	45	55
3	8	12	40	60
4	7	13	35	65
5	5	15	25	75
6	4	16	20	80
7	3	17	15	85
8	3	17	15	85
9	2	18	10	90

* Died.

No. 8.* *U. pugilator,* female

Day	Closed	Open	% Closed	% Open
1	12	8	60	40
2	9	11	45	55
3	6	14	30	70
4	6	14	30	70
5	7	13	35	65
6	7	13	35	65
7	5	15	25	75

* Died.

No. 9. *U. pugilator,* female

Day	Closed	Open	% Closed	% Open
1	12	8	60	40
2	5	15	25	75
3	8	12	40	60
4	3	17	15	85
5	5	15	25	75
6	4	16	20	80
7	6	14	30	70
8	4	16	20	80
9	2	18	10	90
10	5	15	25	75

No. 10.* *U. pugilator,* right handed

Day	Closed	Open	% Closed	% Open
1	14	6	70	30
2	13	7	65	35
4	13	7	65	35
6	11	9	55	45
9	14	6	70	30
10	11	9	55	45
12	10	10	50	50
14	10	10	50	50
16	9	11	45	55
18	10	10	50	50

* This crab was tested at irregular intervals.

The individual records presented above are summarized in table 4.

TABLE 4

No.	Open att. 1st day	Open att. last day	% 1st day	% last day	% gain	Min. open	Max. open	Min. %	Max. %	% gain	No. of days
1	2	19	10	95	85	2	19	10	95	85	10
2	2	17	10	85	75	2	17	10	85	75	10
3	6	20	30	100	70	6	20	30	100	70	10
4	7	17	35	85	40	7	18	35	85	50	10
5	2	16	10	80	70	2	19	10	95	85	10
6	5	11	25	55	30	5	12	25	60	35	7
7	5	18	25	90	65	5	18	25	90	65	9
8	8	15	40	75	35	8	15	40	75	35	7
9	8	15	40	75	35	8	18	40	90	50	10
10	6	10	30	50	20	6	11	30	55	25	10

Table 4 shows (1) that none of the individuals experimented with ever made fewer open attempts than it made on the first day, (2) that the maximum number of open attempts approximates those of the last day, (3) that the greater the number of days an animal was tried, the greater the gain. Number 10, which was tried at irregular intervals, lasting for a period of eighteen days, gained less than any other individual. To be sure, considerable variation in the rapidity of habit formation is exhibited, some of the crabs made their maximum number of open trials about the sixth day, while others did not succeed in learning the direction of the open corner accurately until the last day. Habit formation, like any other character, is subject to the law of variation, being stronger in some individuals than in others.

It is evident, however, that the fiddler crab can overcome his proclivity for one direction, and learn to go in the opposite one, if the latter enables him to escape from a trap. The mere fact that after encountering the glass obstruction, the animal goes to the open side, is in itself significant. The experimenter was obliged to drive the animal away from the closed end in the very beginning of the experiment. After the first ten attempts, it learned to find the opening by itself. But the ability to overcome its inclination almost entirely, making 90% of its movements in the direction which leads to escape, is unmistakable evidence that the fiddler crab possesses associative memory. To be sure, it learns slowly, perhaps more so than the crawfish, but it should be remembered that the crawfish showed no preference for any side, whereas the fiddler crab had to overcome a strong inclination. Viewed in this light the gains which the animals made during the 10 days during which they were tried are enormous.

It should be mentioned that the possibility of establishing pathways was obviated by either scraping off the top layer of the substratum, or by adding a fresh one. Sight and touch seemed to be the chief factors in the habit forming process, the former predominating. It was observed that an animal would often begin to go in the direction of the closed end, but before approaching it, would turn and go in the opposite direction. This seemed to point to sight as the basic factor in the habit forming process.

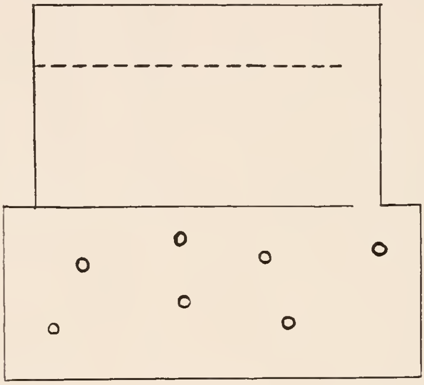

FIG. 2. Showing labyrinth with alley. Dotted line represents glass plates, and circles represent burrows.

In order to test this hypothesis, another type of labyrinth was devised. By means of glass plates, an alley, 10 cm. wide, leading directly to the closed end, was made on one side of the box. The glass plates which formed one side of the alley extended to within a distance of 8 cm. of the end of the box in order that the crab might turn and avoid the blind corner.

Five crabs which had given the best results with the labyrinth test, were made the subjects of the experiment. They were tried for two successive days, being given ten trials per day. Upon being liberated at the end of the alley nearest the experimenter, the crab began to seek a means of escape. If it did not turn before reaching the end of the alley, it was charged with a closed trial. If it did turn, however, and liberated itself from the alley by going to the open corner, the attempt was recorded open. The results are given in the following table, 5:

TABLE 5

No.	Days	Closed	Open	% Closed	% Open
1	1	2	8	20	80
	2	1	9	10	90
3	1	1	9	10	90
	2	0	10	0	100
4	1	3	7	30	70
	2	1	9	10	90
5	1	2	8	20	80
	2	2	8	20	80
9	1	2	8	20	80
	2	2	8	20	80

From the above data it is evident that the animal was guided by its sense of sight in liberating itself from the alley. It should be borne in mind that the fiddler crab is positively thigmotactic, and under ordinary circumstances it has a strong tendency to follow the sides of the box. The animals which were subjected to this test had already learned to free themselves from the trap, and therefore knew the direction of escape. They were, therefore, able to avoid the side which led to the blind end of the alley.

The crabs were given a rest of ten days, at the end of which they were tested for habit retention. Unfortunately, five of the animals died during the interval, and two of the remaining ones were too sluggish to give results. Those which were tested gave the following records, table 6:

TABLE 6

No.	Closed	Open	% Closed	% Open
1	3	7	30	70
3	1	9	10	90
4	2	8	20	80

These three crabs were now tried for unlearning. The corner which had been closed was opened, and the open one closed. Each animal was tried for five days, being given twenty trials per day. The reversal of the open corner was at first very confusing to the creature. Upon being liberated in the chamber, it ran to the closed corner, and remained there with greater stubbornness and persistence than had ever been witnessed before. It was almost impossible to drive the animal in any other direction. Sometimes it would venture to the open corner but would abandon it for the closed one. Gradually, however, it learned to escape, although the effect of previous experience had no influence on the rapidity of unlearning. The records are given below:

TABLE 7

No. 1

Day	Closed	Open	% Closed	% Open
1	17	3	85	15
2	7	13	35	65
3	4	16	20	80
4	4	16	20	80
5	4	16	20	80

No. 3

Day	Closed	Open	% Closed	% Open
1	19	1	95	05
2	13	7	65	35
3	15	5	75	25
4	10	10	50	50
5	5	15	25	75

No. 4

Day	Closed	Open	% Closed	% Open
1	9	1	95	5
2	17	3	85	15
3	10	10	50	50
4	11	9	55	45
5	7	13	35	65

SUMMARY AND CONCLUSIONS

1. The fiddler crab shows a strong desire to liberate itself from a trap, making on an average of thirty-five attempts an hour.

2. It goes persistently to a certain corner, even though escape is rendered impossible by placing glass obstructions to prevent it from climbing out.

3. The hypothesis that dextrous males are inclined to go to the right side, the sinistrous males to the left side, and females equally to the center and both sides, is fairly well borne out by experimental evidence.

4. Taking advantage of the foregoing tendencies, the fiddler crab may be made to reverse its proclivity, and escape from a labyrinth through an opening at the opposite side.

5. It learns slowly, increasing its movements to the open corner with successive trials.

6. The rapidity of habit formation varies directly with the frequency of the trials.

7. Sight and touch are the most important factors in the habit forming process, the former predominating.

8. An animal which formed the habit can avoid a blind corner by turning before reaching the end of an alley.

9. The habit persists after a lapse of ten days.

10. The crab can unlearn the habit, although previous experience seems to have no influence on the rapidity of unlearning.

In concluding, we wish to express our indebtedness to Dr. H. E. Walter for his reading of the manuscript.

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THE ABILITY OF THE MUD-DAUBER TO RECOGNIZE HER OWN PREY (HYMEN.)

PHIL RAU
St. Louis, Missouri

INTRODUCTION

During the summer months two species of mud-dauber are often seen at the edges of streams, filling their mandibles with the soft mud, carrying load after load to some sheltered spot and fashioning it into a many-celled nest. As each cell is completed the wasp provisions it with spiders, usually paralyzed by her sting, cements her egg to one, almost always the last one brought in, and then seals the cell. The egg hatches and the larva spends its time in devouring the spiders while the mother wasp goes on adding cell to cell until the nest grows to great proportions, sometimes as many as thirty-six cells.

Of these two species so commonly seen the steel-blue wasp is *Chalybion caeruleum* and the yellow-legged one *Sceliphron* (*Pelopoeus*) *caementarium.* Our observations are almost entirely upon the latter species. The experiments are for the purpose of ascertaining the wasp's ability to distinguish her own prey or to recognize another's spiders, and her attitude toward such.

In 1912[1] we were watching a Pelopoeus mother industriously filling her cell with spiders. While she was out foraging we borrowed four fine fresh spiders from another new nest near by and with the forceps carefully inserted them into her cell. Upon her return she was at once aware of the intrusion and set about to carry out the foreign spiders with much indignant buzzing. Nor did she stop at this, but carried out and threw away three of her own hard-earned prey as well, before her indignation had cooled sufficiently to permit her to continue her work. It was quite apparent that she recognized the spiders not of her own capture, but why should she reject them because a sister wasp had caught them, and why should she discard a part of her own unless she meant to clear them all out as though they were contaminated ? Would other mother wasps act in the same way under similar circumstances ? These questions led us on to further experiments the following summer, with many varied and surprising results. The observations were made during a week's vacation, on wasps building in an old barn at Lake View, Kansas. Only the details of each experiment can give the reader a fair idea of their varied behavior.

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[1] *Ent. News,* vol. XXIV, pp. 392-396.

EXPERIMENTS

Exp. 1. A new Pelopoeus cell was found already one-fourth filled with spiders. When an opportunity occurred, I slyly filled it high with spiders from another nest. The mother wasp returned with a large spider, and spent some time in laboriously cramming it in. Quite satisfied now with her store, she brought balls of mud and duly closed up the cell. But while she was gone for another load I picked open the seal and extracted part of the contents. Arriving at the nest with the next pellet she saw the injury and was alarmed, hurried out and threw the mud away, returned and indignantly carried out the remaining spiders one by one, her own as well as mine, until the nest was quite empty.

Exp. 2. While Pelopoeus was gone I stirred up the spiders which she had placed in her cell and added one from another nest. When she returned she promptly carried it out, and made four more trips, each time carrying out one of her own capture, until all were gone. Then, after a brief, unexplained absence she came back and inspected the empty cell, fretted and examined and stood guard over it for an hour and a half all because a few spiders had been disturbed.

Upon returning three hours later I found the cell sealed. I opened it and found just two medium-sized spiders, with an egg attached to one. Thus this mother was so anxious about her progeny that she carried out and rejected all of the spiders which had been touched by human hand or forceps, and now she sealed up the egg with only sufficient food to carry it half through its larval life.

Exp. 3. One day while collecting nests I removed a large one from a shelf against the barn-wall. No sooner done than a blue wasp, *Chalybion caeruleum*, returned to it. She examined the spot very carefully for about thirty minutes. When she flew out I replaced the nest, but before doing so I removed five spiders from the new cell which she was engaged in filling. She returned, still with the green spider which she carried when first she missed her nest. She hovered about on the nest very nervously for some minutes and entered the cell five or six times and seemed greatly excited and puzzled; she re-examined the whole nest again and again and re-entered the cell many times, and finally after thus hesitating for about forty minutes she soared away with an indignant buzz, without even depositing her new prey.

While she was gone I removed six spiders from another cell of her own nest (this cell was at the back of the nest, against the wall, so one side was open, but when the nest was returned to its position against the wall no mutilation was apparent to confuse the owner), and placed them in the new cell. She soon returned and set about promptly to remove these six spiders one by one and either dropped them after a flight of a few inches from the nest or carried them quite outside the barn.

Apparently she had had enough of this cell, for after a few minutes she flew in with a pellet of mud and began to seal it up, empty.

Exp. 4. A Pelopoeus mother was busily engaged in stocking her new cell. I plundered the nest of a blue wasp near by and placed six spiders from it in the new cell. The owner returned with a spider of her own, placed it in the cell on top of the stolen booty, pushed the whole in with her head and rammed it down about six times as though it were all her own, then flew out, returning almost at once with a pellet of mud with which she sealed the cell, and reinforced it with four or five more such balls. All this she did with an air of peace and satisfaction in work well done.

If some females can by some sense detect the spiders which have been caught and paralyzed by another of her kind, and express such resentment toward their presence, how much more strange it is that this one does not seem to be aware that part of her prey had been handled by a foreign species entirely, besides myself, or if she does know it, she cares not a whit.

Exp. 5. Next I tried a new form of interference, placing three spiders in a Pelopoeus cell which was only in course of construction, being but one-fourth completed. So it was not at all surprising that the wasp, after a little commotion, promptly emptied this and proceeded with her masonry.

Exp. 6. A blue wasp had completed her cell and placed her first spider there. I removed it, filled the entire cell with spiders from other nests and replaced her own spider in the front of the cell so that she would see her own prey when she returned. However, in handling the contents, I broke out a small piece of the wall at the opening. When I returned in a half hour I found that the cell had been emptied and deserted by the mother. Why did she go to the trouble of emptying it if she meant only to desert it?

Exp. 7. A new Pelopoeus cell appeared complete, but was still empty. The insect brought a load of mud, but used it to reinforce the nest, then she went all the way into the empty cell, and remained there for four minutes, only her tarsi protruding. What may have been her business during this performance we could not determine. When she had gone, thirteen spiders (one with a small egg attached) from another nest were placed in the cell. Upon the second and third trips she also walked over her nest and deposited the mud on the outside to reinforce it; she did not enter the cell, and I did not see her even look inside, but when she again came she used the load of mud to close the cell, then another and another until the seal was firm, just as though all were normal. Whether she detected the ample supply of spiders and closed the cell on that account, or whether she would have sealed it empty, had we not filled it for her, we could not determine.

Exp. 8. A Pelopoeus cell was finished and quite ready for use, but the larder was not yet stocked, so I filled it with spiders from another nest. The mother wasp returned with a fresh spider, started to enter the nest but retreated and flew out of the window, taking her burden with her. She returned empty-handed and carried out the intruders one by one. After the cell had been empty for a half hour, I again placed eleven spiders in it. The next morning when I arrived to examine this cell I found it had again been emptied.

Exp. 9. A Pelopoeus mother was carrying in spiders to fill the twelfth cell of a handsome nest, but had not gone far with the work when I added fourteen from the nest of another of the same species. The wasp returned and at once emptied the cell of my spiders and her own as well, and quietly stood guard over the cell for fifteen minutes with an air of indecision, and then flew away and was not seen again.

Exp. 10. A one-celled Pelopoeus nest was built under a piece of bark on a log beam in the old barn. I carefully removed this bark, filled the cell with borrowed spiders and replaced it. When the wasp returned she had great difficulty in finding the nest. After finally locating it she paused only a moment and dashed away, and returning removed the spiders one by one. Since the position of the nest was disturbed in gaining access to it, I should have been surprised if she had not resented the intrusion, although I cannot understand what caused her great confusion in locating the nest when the alterations in the locality were imperceptible to me.

Exp. 11. A solitary cell contained five spiders when I added six more from another nest. The wasp returned empty, put her head into the cell and worked energetically for three minures, either inspecting or packing them together or laying her egg. Out she came at last and dashed away, but without a spider; almost immediately she returned with her plaster and sealed up the cell.

When she had gone, I broke the seal and removed part of the spiders which she and I had together supplied. She soon returned with another pellet of mud for the seal, but when she found it broken she alertly poked her head in, hastily withdrew and flew away with the mud. After that she made four trips from the nest, each time carrying out a spider which I had failed to remove, but these four were of those which she personally had put in. Then for ten minutes she thoroughly examined the inside and outside of the cell, going in and out many times, apparently in an earnest attempt to discover the cause of the mysterious trouble. When I returned at four p. m. I found her again filling this cell with spiders.

During her absence I again meddled, inserting twelve spiders from another nest. Returning she brought a spider which she crammed into the cell with the others and departed. After ten minutes, however, she came buzzing back as if possessed of a new idea, and commenced to empty the cell. First she took out her own fine new one and threw it away, and returned repeatedly until the cell was again empty. She then remained on the nest, holding watch for thirty minutes, as if resolutely waiting to catch the hoodoo. When she left I expected her to refill the nest with spiders of her own capture, but instead she brought a load of mud and, to my amazement, spread it in a thin layer on the inside of the cell, as though the very walls were polluted, or else all of the trouble were due to its inadequacy. So, for the first time, I saw a wasp adding mud to the inside walls of a cell after she had once deemed it finished.

The next day, August 17, at three p. m., she was still occasionally coming to the cell with an air of angry suspicion and uncertainty, but otherwise it was in the same empty condition that I had left it. Unfortunately I was obliged to leave on the evening train, so I never knew what she finally decided to do.

*Exp.*12 . At six o'clock one August evening I filled a new one-celled Pelopoeus nest with spiders from another nest during the absence of its owner. I was called away and could observe it no further until the next day, August 17, when I found the cell sealed. I opened it and found that my intruders were gone and in their stead were two other spiders. The mother had evidently begun to fill the cell after having thrown out my spiders but had stopped with only two and sealed the cell without having even deposited her egg.

Exp. 13. A certain nest of a Pelopoeus was almost completed when I filled it with spiders from another nest. The proprietress returned with another load of mud to add another ring. When she saw the spiders she withdrew her head with a start, as though greatly shocked. Again she inquiringly put in her head, with a like result. She then went away in bewilderment and returned six times, but each time sought the nest at a spot two feet distant. Sometimes she would walk toward the nest, but always with the manner of one seeking for something lost.

After three days the cell was still in the same condition as I had left it; the wasp never finished it. I think that she firmly believed that her nest was lost, and that the one to which she came again and again was the nest of another which had been filled with spiders.

Exp. 14. At 10:20 a new cell, the fifth on this nest, was commenced, and in just one hour and a half the new compartment was completed and ready to be filled and sealed. At this point I came forward with unasked aid and placed therein fourteen spiders from another nest. The wasp returned with a load of mud, no doubt to put on the finishing ring, but when she saw the spiders she showed not the least surprise or concern, but proceeded to seal the cell with the pellet she had brought. Then she brought another and another and added it to the closing in the normal manner, showing almost human standards of conduct in being satisfied in doing the thing most convenient at hand which gives the appearance of work well done, and glad of the opportunity easily to forget that she had quite overlooked the principal duty of her life. She seemed to give no serious thought to the presence of the spiders, nor did she make an effort to compress them nor show any concern for depositing her egg. The sight or scent of the spiders seemed to afford sufficient stimulus to cause her to seal the cell. Perhaps the presence of the mud already in her mandibles lent strength to the stimulus for this particular action.

At four o'clock that afternoon I found that this industrious mother had made another cell and was finishing off what I thought must be the last ring. When she flew out I placed six spiders in the cell and had not time to insert more when she returned with another load of mud. She got a glimpse of the spiders, which in this case only half filled the cell, and almost immediately flew out with the pellet. She threw away her mud and came hurrying back, peered into the cell and then bustled out again. She came back to the cell bent on her course of action, got a spider and carried it out. I then hurried to completely fill the cell by adding ten more spiders. But her zeal for righting wrongs was now aroused, and even this was no inducement to seal it up, for she carried them all out one by one.

Exp. 15. The new cell on this nest was just completed but as yet contained no food supplies, so I placed in it eight fresh spiders taken from another nest. The mother wasp returned with a load of mud and alighted on the nest, but from her behavior I judge that she suspected that it was not hers, for she arose on the wing and flew in wide circles and returned. This she did three times, the last time making a good many smaller circles. Through all of this confused search she carried her pellet. By this time she seemed fully convinced that this was her home, but that something was wrong. So she dropped her ball of mud out at the window, returned in a direct line to the nest, and began with a very positive air to carry out the spiders one by one, throwing them away until all were gone.

Exp. 16. A wasp was discovered putting the first layer on the closure of her cell. I removed this and also part of the spiders, all of her own capture. The wasp came in with more mud; hummed a little in anxious concern and flew out with her load. She returned shortly, however, and again sealed the cell. Again I opened it and inserted other spiders from another nest. She came back and saw the opening, poked her head into it enquiringly and proceeded to plaster it up. For the third time I broke the cell, but she seemed inclined to repair it as long as I would continue to damage it.

Exp. 17. A wasp had packed her cell nicely and already sealed it with two layers of clay. I carefully removed the covering and part of the spiders. The wasp returned with the next load of mud, hesitated only a little and spread it in its proper place and was off again. Again I opened it and this time inserted four foreign spiders. In due time the mother returned and again plastered the opening as if nothing had happened and departed. Bent on commanding her attention I broke the seal for the third time and placed a larva of Pelopoeus in the doorway, half protruding, so she could not seal the compartment without removing it. By the time she arrived with a pellet this larva had worked itself out of the cell, so she spread the mud as usual over the cell. When she had again gone I tried another very large larva in the same way. The mother wasp returned, made no attempt to remove the larva, or in fact displayed no concern for its presence, but spread the mud around it as it lay half protruding from the cell, often severely jarring it as she worked, plastering her mud to the sides of the larva as though it were a part of her wall, and thus again sealing in this silly fashion her cell.

Exp. 18. To a Pelopoeus cell containing a few spiders I added five from another nest. The wasp returned, carrying another spider which she crammed into the cell, while with her head she condensed the whole mass. In so doing she somehow dislodged one and it fell into the spiderweb below; she alertly recovered it, crammed it into the cell with precision and continued to pack the mass together for about five minutes, then flew out and brought one more spider which she deposited, almost filling the compartment.

When she had gone again I forced five additional spiders into the cell; after a half hour she returned with another capture which she also forced in with great effort. It seemed that she had a fairly definite idea how many spiders were required, and bring them she must and would, regardless of unsolicited aid. In this she differed from other individuals of her species, in whom the sight alone of few or many spiders in the cell was sufficient stimulus to induce the sealing process.

But upon her next return she brought a load of mud and closed the cell. When she was gone I opened it and removed one-third of the spiders. The next load of mud was used in precisely the same way; absolutely no attention was paid to the broken cell or the missing spiders. Again I removed the seal and all of the spiders in order more forcibly to impress upon her the seriousness of the injury. I accidentally broke a small piece out of the wall of the cell at the opening. The wasp returned, spread her mud over the opening, leaving the broken part untouched and quite ignoring the emptiness of the nest or the traces of vandalism. She discharged her duty always with a mechanical faithfulness; she seemed, nevertheless, exact—three loads of mud are usually required to seal a cell, and three loads she brought and applied properly before finally leaving the nest.

Exp. 19. When I arrived upon the scene the fourth cell of a Pelopoeus nest was half filled with spiders. Not having other spiders at hand, I placed a pupal case containing a pupa of the same species in her cell so that no part protruded. When I returned two hours later the cell was sealed and a fifth cell of the nest half completed. I had to break open the cell to see if the pupa had been removed. The cell was quite empty, but the new item of interest was that at six the next morning I found this damaged cell repaired and the fifth cell still in its half finished condition. This was the first case in my experience of a wasp going back and giving attention to a previously finished cell after a subsequent one had been begun.

CONCLUSIONS

When we attempt, finally, to formulate any generalizations concerning the behavior or psychology of these insects, there seems to be only one principle which can be relied upon to hold good in all cases, viz.: that the madam will do as she pleases. Cases of similar conduct under homologous circumstances can hardly be found. Yet we cannot regard the behavior of the wasp as indifferent or accidental when we see her very positive air in taking action, and her usual determination and persistence in pursuing it when she has decided upon her course of action. It may seem to some readers that these observations are too artificial or experimental in nature and too limited in number to justify a conclusion so vague. To be sure all these experiments threw the insects under abnormal and unnatural conditions, so we need not marvel, perhaps, that no two behaved alike under provocation. But the detailed examination of many hundreds of completed nests[2] shows that in normal, free life these wasps commit blunders or follow disastrous whims in a large proportion of their cells; sealing them stark empty or with only a fraction of the food necessary for the young one, or providing abundant supplies and omitting the egg, or other blunders which would defeat the whole purpose of the wonderful instinct of nest-building.

In answer to the question suggested in the title we can only say that in most of the cases where the spiders were disturbed the owner was quick to detect it and frequently resented it. But since in her anger she often threw away part or all of her own prey we cannot determine whether or not she recognized her own, or merely regarded with alarm any meddling about her home. Likewise in those cases wherein she accepted our proffered aid she did so with such outward indifference, taking it all as a matter-of-course after the manner of those accustomed to welcoming charity, that we could not discern whether or not she was the wiser.

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[2] The data are in course of preparation for publication later.

OBSERVATIONS ON THE BEHAVIOR OF BUTTERFLIES

CHARLES W. HARGITT

The following observations have been made at various times during several years as opportunity has afforded, and with little thought that they might ever be offered for publication. Looking them over recently it has seemed that there might be a few sidelights which would have some interest to students of behavior, and with this in mind they have been written out quite briefly as an incidental contribution to a subject of vast interest and importance.

The lepidoptera have been for many years a favorite group among students of tropisms. The familiar phenomenon of the moth fluttering in the candle flame at night has long ago passed into a proverb. It is only within recent times that observations upon butterflies, and also upon many larvae of these forms, have come in for critical study and attempted explanation. It is no part of the present purpose to attempt any review of the subject, though a few references can not be avoided in discussing the facts to be reported. While the earlier observations and deductions of Loeb, Davenport, Graber, Radl and others have been of value, and have stimulated greatly the interest in the subject, it remained for later students to undertake to study with accuracy and critical control the factors involved in the behavior. To the writer it has seemed that the work of Radl and Parker have been noteworthy in this respect. It was the graphic account by the latter on "The Phototropism of the Mourning-cloak Butterfly, *Vanessa Antiopa* Linn.," which prompted the observations herein submitted. In most respects they will be seen to confirm the facts cited by Parker, and but for a few features which apparently differ in certain fundamentals, there would have been small occasion for giving them publicity.

Let me say at the outset that my observations were made wholly in the open, that is, in the natural habitats of the organisms, no attempt being made to put specimens under artificial control. In earlier papers I have expressed the conviction that much of such artificial work has been far from convincing, and some of it actually mischievous. It may be probable that some of this failure attaches to study of lepidopteran behavior !

My observations began with the study of *Vanessa antiopa*, and were chiefly directed to that species, but several other species also came in for a share of attention. The following points will be emphasized: (1) Marked individual differences of behavior under apparently identical conditions; (2) differences at various times of day, and various days; (3) marked sense of locality and adherence thereto; (4) lack of evidence of any sex adaptation in the color markings as related to behavior.

My observations confirm those of Parker, (1) as to the dominance of "chemotropic response to food;" (2) the general negative phototropism in strong sunlight; (3) general indifference of butterfly to shadow stimuli except in the head region.

My first notes on the behavior of Vanessa were made on a bright, warm day, the 25th of March. The first two specimens found were very wary and difficult to approach, but two other specimens proved less wild, and allowed easy approach and close observation and experiment. Several others were found later which also allowed approach and similar observation. One of these specimens alighted on an exposed snow-bank, oriented in the usual manner, seemingly not at all disturbed by the icy substratum on which it rested. In all some twenty careful observations were made in relation to the particular orienting behavior, and in general conformed fairly constantly to the results obtained by Parker. As a basis on which to estimate the degree of exactness of the orientation I regarded any reaction which did not vary more than ten degrees from the precise line of the sun's rays as conforming to the law, while anything beyond this was regarded as a departure, or failure to conform to the law. This is, of course, a purely arbitrary way of estimating the reaction, but unless one insists on mechanical precision in every case (a method which might be demanded), it seems as good as one might propose. In the first series, just given, the majority clearly behaved in conformity with expectation, but a number as clearly fell outside such expectation. In this connection were noted facts which clearly illustrate the individual difference of behavior, e.g., the differing susceptibility to alarm. Shall one use the term *alarm* in referring to such behavior ? If the organism is pure mechanism the use of such terms is of course inadmissible. But if we are dealing with an *organism* in the true sense, then no other term is more pertinent and significant. That this is the real state of the case one is forced to believe in that the same specimen will exhibit the same differences of behavior at different times, acquiring keener sense of alarm from *experience*. Again, the behavior varies on different days. On some days they seem to seek the ground predominantly, while on others they " come to earth " seldom and for brief periods. This was noted so often as to leave no doubt as to the fact. That it may not be due to difference of light intensity is evident in that the same differences will be observable in different specimens at the same time and therefore under identical light intensity. For example, it was found to be true in the behavior shown at ten o'clock and that at two o'clock the same day, and under indistinguishable conditions of light, though appreciable differences of temperature were evident, and it is not impossible that this may be a factor in the matter. Exactly these facts were illustrated by my next field trip four days later, on March 29th. In the forenoon specimens were extremely wary and difficult of approach, and the behavior was erratic and uncertain. During the afternoon of the same day, accompanied by an assistant, it was like encountering a different species. Specimens were " tame," observation was easy, and any number of tests could be made with precision.

My next observations were made just a month later, with a clear, warm day. At least seventy-five observations were made during the afternoon, including numerous shadow tests. While in the majority of cases there was a more or less evident orienting response, as before a considerable number varied greatly as to the precision of reaction. It was not unusual to have a specimen alight at an angle of 90 degrees from the parallel of the rays of the sun, and occasionally a specimen would come to rest directly facing the sun and remain thus. A single specimen was found which proved very approachable and responded very readily to tests, and on it were made about forty direct tests, of which thirty showed orienting reactions more or less precise. The other ten reactions showed considerable more deviation, sometimes as much as 45 degrees. In course of these observations it was found that the position of the support upon which the specimen came to rest often had a modifying effect as to its final position. That is, if the specimen alighted upon a twig which was slightly out of the line of the rays of sunlight it conformed to the axis of support instead of that of the rays. In one case the specimen alighted upon a dry leaf stem with the head upward, and about 30 degrees from the parallel of the sun's rays. This effect of the influence of the supporting basis was frequently observed in later cases and I think affords an important factor to be taken into account in such cases. Evidently here was a stimulus which served to modify in a very appreciable degree the character of the behavior., On this specimen a number of experiments were made by means of shadows cast upon the body. In some cases these were produced by means of the hand, sometimes by using one's hat, and in some cases by a cane which might be made to cast a definite and localized shadow. Under total shadow the specimen usually showed reaction in from 5-10 seconds, and within 15 seconds would fly into the light (occasionally the movement would be by crawling). Under partial shadows, i.e., a part of the body in shadow, the reaction was less prompt, from 15-20 seconds. As in the total shadows, the reaction might involve flight, or a mere creeping forward or sidewise, as the case required. The response was in general more prompt with the shadow on the anterior of the body and head and slowest when the posterior part was under shadow, which would seem to imply the relation of sight in the reaction, though not wholly.

On May 27th a series of observations were made under very favorable conditions, the specimens being easily approachable and seldom taking fright or leaving the place. The records of the day included fifty observations, and of these hardly more than half of the photic reactions came within the 10 degrees arbitrarily set as a sort of limit for precise orientation. Variations from 10-30 degrees were very common and in a few cases the variation was definitely 90 degrees from the line of the rays. Experiments with shadows showed some interesting features not noted before. In a few cases total shadow produced no reaction at all; but in most cases there was response within about the limits already noted. In some cases a specimen would give signs of reaction by becoming restless, edging sidewise, forward, and finally in flight, alighting in open sun. One of the unusual phases intimated above was noted upon two specimens, namely, while resting and oriented in about normal manner, with wings spread wide and flat, they would slowly close them over the dorsum. While in this pose, if a shadow were cast upon the specimen, its reaction would be an immediate spreading of the wings. Upon removing the shadow the wings would again close over the back; and repeating the shadow the same reaction would occur. This experiment was repeated upon one specimen seven times at intervals of from 15-20 seconds.

To test whether this particular form of reaction was due to sudden visual reflex the interposition of the shadows was made so gradually as to render any such reaction rather improbable, or again by sudden thrusting of the shadow upon the body to induce such reflex. But it was not evident that the reaction was wholly visual.

Another feature of the observations today was the fact that no selection was apparent upon the part of specimens as to the place of coming to rest. For example, they frequently came to rest on the open, spreading leaves of the mandrake, leaflets of cohosh, grass blades, etc., and in some cases nestling down among grasses, utterly indifferent to the hazy shadows of such positions. In other cases they would alight on dead stumps, naked limbs, flat stones, etc., and almost invariably with the head directed upward, sometimes at an angle of 20-30 degrees from level of the ground. In only one case in all the observations was a specimen seen to alight upon a tree trunk.

Sense of locality. It was frequently noted that a given specimen showed some sense of particular locality. For example, it was often observed that a specimen at rest and oriented in a given place would arise in chase of a passing specimen and after a buffeting flight together for some distance the first specimen would return and alight in the same spot from which it had taken flight. This was seen so many times that there hardly seems doubt of the fact that it reveals a sense of locality almost as marked as by such insects as bees. Reference will be made to this matter in a later section in connection with the question of sex attraction.

My next observations of importance took place at Woods

Hole in July and early August and had to do with another butterfly, namely, *Argynnis idalia*, a species rather common in the locality. The specimens are of the field habit rather distinctly and seek the open sunshine. Like Vanessa this butterfly orients itself in almost exactly the same manner. But their reaction is much less exact than that of Vanessa. And when tested as to the effect of shadows to my surprise they showed hardly any reaction. In a number of cases the shadow of a hat interposed and withdrawn as many as a dozen times at intervals of from a few seconds to as much as a minute produced no response. These experiments were repeated on other specimens and with the same results. When put to flight a specimen soon comes to rest in the same general attitude as before. The color of this insect is much more striking than is that of Vanessa, and if this were a means of attracting mates, as Parker has suggested, then it might be expected to be much more effective. But I have never, in either case, seen the slightest evidence that this is in any sense such a device, nor that the special pose and orientation has anything to do with such ends. As with Vanessa specimens of Argynnis show great variation as to ease of approach, some being exceedingly wary and wild, others tame and easily studied. Such is the case with almost all the species studied. Whether this may be due to greater or less visual sensibility or simply to more or less alarm in the presence of strange objects may be matter of doubt.

My next observations which add anything essential to the facts concerned were made in September of the following year in the fields adjacent to Syracuse. I had at this time opportunity to observe several species in addition to Vanessa antiopa, among them a species of Papilio, probably *asterias*, and another which I was not able to identify. As compared with Vanessa the behavior of Papilio showed several rather marked differences. In the first place there was no indication of phototropism of any sort. On coming to rest upon the ground there was not the slightest disposition to orient itself with reference to the sun's rays. On the other hand there was orientation with respect to direction of wind, the creature seeking to face the wind thus probably taking the position of least resistance to the wind which was rather strong at times in the exposed field. Continued observation showed that this behavior was not merely incidental, but definite and purposeful. In flight there was no apparent reaction of the sort, the specimen flying as much directly against, as with the wind currents. In repose the specimen showed the same pose of wings as Vanessa, a fact which was rather unusual for a Papilio, whose attitude is usually quite the opposite, namely, to rest with closed wings. The response to shadows was essentially the same as Vanessa, though less marked At times a specimen would remain at rest indefinitely under a shadow, but the opposite reaction was predominant.

The other species behaved in much the same manner as Vanessa, but its photic reactions were much less marked. Its behavior in relation to other species in flight was exactly as in Vanessa and other species already mentioned. This chasing and buffeting behavior appears to be related to the mating instinct, but it was not possible to distinguish that it ever resulted in actual copulation. Further reference to this will be made in another section.

Numerous other observations were made, all giving about the same results, and all revealing more or less clearly the individuality to which attention was directed in the introductory section. It was quite evident that in this behavior one has to recognize that reactions are not simple, nor are they definite and stereotyped as might be expected on the assumption of the so-called laws of phototropism. As Parker has well said, " this problem, at least so far as butterflies are concerned, is much more complex than was suspected by either Loeb or Davenport. The reactions of Vanessa antiopa to light cannot be satisfactorily considered without dealing with the influence of heat, food, and gravity." I think it may also be added, without recognizing the influence of an individuality characteristic of all higher organisms.

Sex as a Factor in Behavior. Parker has emphasized the probable relation of certain phases of the behavior, especially that of the peculiar pose of the wings and photic orientation, to the problem of " bringing the sexes together during the breeding season." This view has received no confirmation in my observations. At no time have I ever observed a specimen in flight hover about one in repose as if attracted toward it. Invariably the first sign of recognition has been by the resting specimen, which often appeared to be on watch for the passing of one of its kind. This was true of all the species observed. I have often noted the fact that any passing object in flight over one of these "watching" specimens, such as a bird, or a bumble bee, would have the effect of stimulating the same sort of chase as would be the case with a similar passage of one of its own species. I have seen a Vanessa chase a Papilio, or a Pieris, or, indeed, almost any similar object.

That there is a sex factor involved in this peculiar behavior I think altogether probable. But that the color pattern, or the wing pose of the specimen has any such function seems extremely improbable. A further fact which tends to support this view is that the behavior in question does not seem to be limited at all to the breeding season. It is quite as marked in July as in April or May. Indeed, so far as my observations go, there is nothing to show that this behavior differs materially at any time during the active life of the butterfly.

Still a further point may be noted as bearing on the question, namely, it does not seem to me that the color pattern of the wings of Vanessa serve to make it a specially conspicuous object when in this orienting pose. If a perfectly bare, white or grayish position were always sought this might be the case to some extent, but the habit of Vanessa rather dominantly in or about wood lots, where many and varied lights and shadows mingle, would tend to render these markings rather protective than otherwise. I have personally demonstrated this on many occasions when following up a specimen for closer study. Even when marking down a specimen as it came to rest and hastening forward with the eye upon the spot, it often was impossible to see the thing until it took flight, so intimately had its markings been blended with its surroundings.

These facts and the further fact that the behavior is not peculiar to Vanessa, but is shared by a considerable number of species, some of which are very brilliantly colored, afford a strong evidence in disproof of the view proposed by Parker touching its function as a sex factor.

NOTES

THE ROLE OF THE EXPERIMENTER IN COMPARATIVE PSYCHOLOGY

ROBERT M. YERKES
Harvard Psychological Laboratory

In Comparative Psychology attention has recently been concentrated upon the control of the experimental situation to the neglect of two other aspects of our task which are equally worthy of study,—namely, the management of the subject and the reliable recording of responses. Every psychological problem presents, if attacked experimentally, these three technical demands: first, such control of the objective situation as shall render it not only suitable for the solution of the particular problem, but at the same time, highly controllable and describable; second, such knowledge of the subject, human or infra-human, as shall enable the experimenter to avoid unnaturalness, or otherwise unnecessary ill-adjustment of subject to objective situation; and third, such provision for the recording of response as shall provide wholly reliable and sufficiently detailed descriptions of the subject's behavior.

By experience in working with various animals and with pathological human subjects, I am convinced of the urgent need of attention to our methods of recording reactions. We, at present, allow the experimenter too great range and place upon him over-great responsibility. As observer, he is liable both to influence the subject in his attempts to get data of reaction and, in turn, to be influenced, in his descriptions of what he sees, by his unescapable tendencies to interpret. Quite evidently, the ideal experiment is one in which the subject provides us a detailed photographic record (or other form of graphic record) of its response. It is the writer's belief that we should make systematic and persistent attempts to develop recording devices which shall free us from the observational imprefections of the experimenter.

This means that our apparatus for use in Comparative Psychology must be largely automatic or self-controlling over considerable periods of time, not only with respect to the objective situation or setting in which the subject reacts, but also with respect to the recording of the several important aspects of response. We should devise types of recording mechanism which shall either operate automatically or be operated by the subject rather than by the experimenter. This would mean not the elimination of the observer but the freeing of his attention f or those aspects of the total experiment which most urgently demand control.

As an example of a practical recording device, I may mention that of the Hamilton Quadruple Choice Apparatus which, in its latest improved form (thus far undescribed) permits the experimenter to confront his subject with a certain situation and then leave that subject to work out a series of problems, its behavior in connection with which is the while accurately recorded by a system of markers, electrically actuated.

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THE INFLUENCE OF DIVERTING STIMULI DURING DELAYED REACTION IN DOGS

ARTHUR C. WALTON

From the Zoological Laboratory of Northwestern University

INTRODUCTION

The experiments recorded below were undertaken to determine the effect of various diverting stimuli applied during " Delayed Reaction " in Dogs. The diverting conditions were either employed in connection with the primary stimulus, or were interposed during the delay period. While the diverting conditions disturbed the " Orientation Clues " to which Hunter (13) has attached much importance, at the same time they may be used to determine the power of the dog to retain the original stimulus under complex conditions, resembling those which prevail in natural behavior.

The observations were carried on during the year 1913-14, at the Zoological Laboratory of Northwestern University under the direction of Dr. E. H. Harper, to whom the writer expresses his thanks for assistance, suggestion and criticism of the manuscript.

DESCRIPTION OF THE DOGS: THEIR TRAITS AND NATURAL CAPACITIES

The dogs were brother and sister of a litter of four, the results of mating an English bulldog and a Scotch Collie mother. Both had the brown coats and white vests of shaggy hair that is characteristic of the Collie, and the male was a collie in build, while the female had the short bowed legs of the bulldog. The female was fawning and easily diverted. Her attention was mainly towards the experimenters, and rarely towards the problem, unless the experimenter was entirely concealed. This disposition was not conducive to the giving of good attention, and, as her seemingly dainty appetite caused the " hunger stimulus " to lose its potency, her attention was so poor that results from only the simplest associations could be obtained. The male was affectionate, but never fawning and, as the "hunger stimulus" was very strong in his case, his attention was very good in the more complex problems as well as in the simpler ones. For these reasons the results set forth in this paper are based almost entirely on the records obtained from the male.

The native disposition of the dogs was to shrink from the electric light bulbs employed in the experiments, and only long training would lead them to overcome this aversion to any place thus indicated. The artificiality of the light stimulus is unquestioned, and to react to it, the native fear of the dog must be overcome. For this reason reaction to light stimuli cannot, as some experimenters have claimed, be taken as indicating the true native capacity of the dog. It seems more logical to conclude that only the reactions to stimuli that are naturally attractive to the dog can be taken as true indices of its native capacity.

The health of the dogs during the whole period of the experiments was good. Occasionally slight indispositions were shown. The dogs were kept out of doors, and were allowed to run with other dogs and play with the children of their owner. This life prevented the acquisition of any characteristics peculiar to housed animals.

DESCRIPTION OF APPARATUS AND METHODS OF THE EXPERIMENTS

The experiments were given in a room that was kept partially darkened in order to have the light stimuli in a strong contrast to the general lighting of the room.

The apparatus is illustrated in Fig. 1.

The release box (" B ") was thirty inches wide and forty inches high with a glass top measuring fourteen by fourteen inches. The door, made of wire screening, occupied the whole of one side, hinged at the top and provided with a counterpoise which enabled it to swing easily. The box could be rotated very easily on its " domes of silence." This release box stood

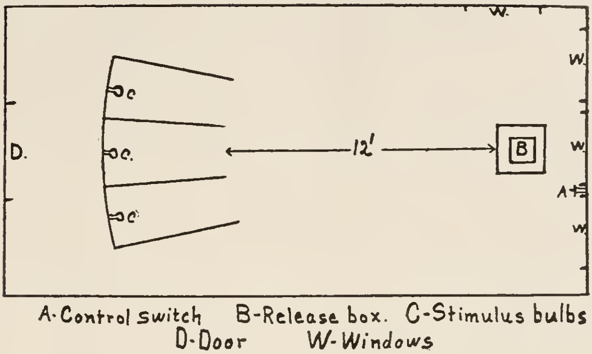

FIGURE 1

twelve feet from the mouths of the food compartments, arranged so that the mouth of each was an equal distance from the release box. The compartments were five feet deep and two feet wide and contained the food bowls. These bowls, during the latter portion of the experiments, were kept filled with water, so that the dog never suffered from thirst. The compartments were at first of wire screening (two inch mesh), but in the four light experiments the walls were made of muslin. In the two and three light experiments the lights were placed at the rear end of the compartments, but in the four light experiments they were placed over the entrances. This gave an equal stimulus from each compartment, keeping them all on a level. The fourth compartment had the approach to the food bowl of a fourteen inch board with cleats nailed to it, set up at a 30 degrees angle, and leading to the food bowl on a platform thirty inches from the floor.

The method of the experiments was at first to put the food in the bowl, switch on the light for five seconds and then off again and release the dog at the end of the delay period. Later the method omitted the placing of the food in the bowls. At first two observers were always in the room standing behind the release box so as not to be visible to the dog during his trip to the food compartment. While one handled the apparatus, the other gave the reward and kept the records. In the early tests, the food was placed in a covered bowl in one compartment, the bowls in the other compartments having been smeared with the food to avoid a special olfactory cue to the correct compartment. Later this method was dropped to avoid giving visual or olfactory cues as to the position of the reward. The food was now thrown to the dog after the completion of the correct reaction, but the dog developed a lax habit of only partially completing a reaction. He would approach only to the entrance of the food compartment and then await the reward. To avoid this result a new method of giving the food reward was introduced. One experimenter stood outside of the door (D—Fig. 1) watching the dog's actions through a peephole and entered the room with the reward only after the animal had traversed the entire length of the correct food compartment.

The experiments were given under varying conditions and the results are recorded separately for each type under the following heads:

Condition "A"—Light Stimulus—Operator in view of dog. Release box faced compartments all the time. Used in the training experiments with Two and Three Lights.

"A1"—Food placed in the compartment at the beginning of the trial.

"A2"—Food thrown to compartment after a correct reaction.

Condition "B"—Operators out of sight of dog until after reaction was complete. Release box faced the compartments during stimulus and delay periods. Used in delay experiments with Two and Three Lights.

"B1"—Food placed in compartment at outset of trial.

"B2"—Food thrown to compartment after a correct reaction.

Condition "C"—Light Stimulus—Operator out of sight of dog until after reaction was complete. Release box turned 90 degrees during delay period. Used in "Two, Three and Four Light Experiments."

"C1"—Food placed in compartment at outset of trial.

"C2"—Food thrown to compartment after correct reaction.

"C3"—Operator out of room until reaction is complete. Food given after correct reaction.

Condition "AD"—Used in "Two Light" experiments. Disturbing stimuli (Olfactory, Auditory and Visual) given during delay period. Release box faced compartments during entire reaction. Food given after correct reaction.

Condition "CD"—Used in "Two Light" experiments. Disturbing stimuli (Olfactory, Auditory and Visual) given during delay period. Release box turned 90 degrees during delay period. Food given after correct reaction.

In the early experiments the order of trials was prearranged, and followed strictly during the trials. However, in the later trials a given experiment was repeated as many times as necessary to secure a successful reaction. Otherwise the prearranged order was followed out. Care was taken that the order of trials should not be such that a possible rhythm could be followed. Record of attention to the stimulus was noted by means of watching the dog through the glass top of the release box.

Any particular orientation during the delay period was watched for and noted. The path to the food compartment was recorded and hesitations or wide turns were especially noted.

The designation " Two Light," " Three Light " and " Four Light " experiments refer to those experiments in which the stimulus was given in one of the possible two, three or four compartments and the dogs were forced to discriminate the correct one of these several possibilities.

In the early experiments the trials were given in series of five and ten, and no advance was allowed until 50% of a series were correct reactions. Beginning with the " Two Light " experiments, and continuing through the rest of the work, no advance was made until at least five successive correct reactions were obtained.

At first the dog was called back to the release box after a trial, if he did not return at once of his own accord. It was noticed, however, that after a number of unsuccessful trials, the dog would refuse to leave the release box on the opening of the door. The dog at the beginning of the " Two Light " experiments returned so regularly that it was decided not to call him back anymore. After this change the dog would lie down for a minute or two, if discouraged by several unsuccessful trials, and then would return to the box with renewed energy and with his attention to the problem as keen as ever. He did not refuse to leave the release box any more after this change in the method was made.

The time of the day when the trials were made varied from 8 to 11:30 A. M. As an interesting sidelight it may be noted that the time from 10 to 11:30 A. M. was the period in which the dog was the most attentive and eager, hence giving the best results. In considering the records, the difference in the time of day has not been considered of enough importance to be mentioned in each series of trials.

The system of retaining the stimuli at one compartment until the dog reacted towards it correctly, begun in the " Two Light " experiments, was continued in the new trials, with the " Three Lights," thus the compartment with the poorest association received the greatest number of trials. While this savored of the trial and error method, yet it was the most successful method that could be found. It was in fundamental accord with the noticed behavior of the animal in learning the association to any one bowl or food compartment. He had to learn to react to each compartment separately, and thus set up the habit of going to that one when he received the proper stimulus. This method gave immediate results as compared with the method used at first of a prearranged schedule of compartments, and no repeating on a failure. There, in the case when the cue to "number two" had been lost, it took a series of eighty-one trials, twenty-seven of which were on " number two," to regain the lost cue and respond to it ten times in succession. By this latter method the association is generally set up again within two or three trials.

The question of " Punishment and Reward " has been a very important one to observers of animal behavior in the higher forms of life. After the preliminary experiments it was decided that a punishment, after an unsuccessful reaction, other than that of losing the food was out of the question. The dog lost interest and became afraid to try for fear of punishment. That the loss of food was in itself a severely felt punishment was already shown by the sheepish action after a failure. His tail dropped between his legs and he sneaked back to the release box with his head down. It took several successful trials to entirely lose his sheepish manner.

RECORD OF EXPERIMENTS

The dogs experimented with were not entirely unfamiliar with associating the required reactions with the stimuli, for Dr. Harper had trained them somewhat along such lines in working on another problem. Thus the writer was able at once to begin with light association experiments. Dr. Harper had found that by the use of very attractive stimuli, such as (1), the smell of meat, (2), waving a handkerchief, and (3), a whistle call, the dogs gave very close attention and could bridge delay periods of five seconds duration.

The work of the writer falls into two main heads, (1), the " Training Experiments," to establish the Light Association and (2), the trials of " Delayed Reaction."

1. Training Experiments.

These experiments were given to familiarize the dogs with the electric light as a stimulus to reaction, in addition to the ones they had already learned. This was done by combining the light stimulus with a familiar one and gradually dropping out the familiar one and leaving only the lights as a stimulus. When the dogs came to react correctly to the stimulus when it remained during the entire trial, the delay periods were introduced in which the dog was forced to make the reaction when the stimulus was absent. These trials were recorded separately from the delay trials only in the " Three Light" experiments. While some such trials were given during the " Two " and " Four Light " experiments, they were too few to be discussed under this separate head, but are mentioned at the beginning of the corresponding " Delay Experiments."

"A" A1—The series of trials of this type extended over the period between October 4th, and November 19th, 1913. The curve representing the learning period for the discrimination of the three compartments was very short and steep. By October 25th, perfect mastery was gained. The results of the series up to October 25th are as follows: 50%, 25%, 25%, 73%, 80%, 90%, 80%, 100%, 100%. Every succeeding series, totaling eighty trials, gave 100% results. As stated before, the choice of the order of the compartment to be used was determined in advance of the series and care was taken to avoid any possible simple rhythmic succession of choices.

A2—To avoid the possibility that the dog was gaining cues to the proper reaction from the olfactory stimulus of the food in the bowl, the problem was again taken up on January 12th, 1914, almost two months after the former experiments had been stopped. In these trials, the food was not given until after the reaction had correctly taken place. Four series (twenty trials) from January 12th, to February 9th, 1914, were given and all showed 100% results, proving that the olfactory stimulus was not necessarily a factoring one. During the same time, check series were held to test the possible rhythm of the dog's choice of compartments. A series of twenty-five trials was given on No. 2 compartment, and one of ten trials on No. 1. The first series gave 80% of the results correct, and the second showed all the trials correct. These trials show that rhythm was not a factor in the dog's choice and that the errors were due to other causes. The habit of going to a different compartment for each choice was so strong that it forced the dog at first to another compartment in spite of the stimulus calling to the same compartment again. However, but five of the thirty trials were mistaken in that way. That this rhythm of going to one compartment did not form a habit was shown by the fact that a return to the discrimination of the three compartments showed no errors or even hesitations. These results show that the dog can gain perfect mastery of a problem involving the discrimination of three compartments when the light stimulus is present at the time of reaction. The fact that rhythmic and olfactory cues are not strong factors in successful reactions is also shown.

"B"—Condition "B" means the release box faced the food compartments only for the stimulus, so that the dog could not gain any possible cue from seeing the operators go to the compartment. Only trials of the "B2" type were given, the food reward being given after the reaction was completed. Seventy-one trials of this type were given preliminary to the undertaking of the "Three Light" experiments in condition " B." Thirty-nine, or 55 %, of the trials were correct reactions, and indicated enough of mastery to favor the adoption of the delay periods of the " B " type.

II. Delay Experiments,

"*Three Light Experiments.*"

Condition "A"—Light out at release. This condition meant that the light stimulus of five seconds' duration was present up to the moment of the release. Thus the entire reaction was performed after the stimulus had been removed, but the association was forced to bridge the gap between the instant of release and the time of the choice of the compartment. As before, the two phases, A1 and A2, depending on the manner of giving the food, were recorded separately.

A1—Ninety trials of this type, i. e. having the food placed in the compartment before reaction, were given and seventy-one were correct. These trials extended from October 18th, 1913 to November 19th, 1913, and show that practical mastery had been gained. Perfect mastery could never be attained because the factor of attention in the dog was variable and, hence, if he did not see the stimulus, at release, he was without a cue and depended on chance. In the former tests, perfect mastery was shown because the stimulus was always there and could not be missed when leaving the release box. Several series of five trials were all correct, but longer series showed at best, only 80% of the reactions as correct.

A2—In order to test the memory of. the association formed, and also to avoid the possibility of olfactory stimuli from food in the compartments, trials in which the food was given after correct reactions were taken up on February 9th, 1914, and extended over to February 16th, 1914. A series of ten trials gave 80% correct reactions and showed that the association was not based on olfactory cues in any appreciable extent. The question of rhythm kept coming up as a possibility, so check trials were given on each of the three compartments to see if this change of rhythm would cause a falling off of results, and if such a falling off should come, to see if changing the rhythm was the cause. Forty trials were given on No. 1 compartment with only 50% of correct results on February 12th, 1914. This falling off in results cannot, however, be laid to the changing of the rhythm. Just previous to the trials, the dog had run into No. 1 when the light was on and had touched the hot electric bulb with his nose and received a slight burn. As a result he avoided No. 1 constantly for the first ten trials, and for five of the next ten. The last twenty trials showed fifteen correct reactions of which the last five were successive. The later mistakes were due rather to the habit of changing, than to the following of any rhythm. A check of ten trials on No. 3 compartment followed on February 14th, 1914 and 60% were successful, the last five being in succession. The four mistakes did not follow any rhythm for the dog went to No. 2 three times in succession and the fourth time to No. 1.

The order of his choices on No. 3 compartment were 1, 3, 2, 2, 2, 3, 3, 3, 3, 3. The habit of changing from one compartment to another had to be broken up before the dog would go to any one successively on the proper stimulus. A check of fifteen trials on No. 2 compartment gave 73%, or eleven of the trials as correct reactions, the last five being in succession. All of the four mistakes were on No. 2, which was the compartment on which the previous check had been held. These results show that *habit* rather than *rhythm* was the factor that governed the successes of the check trials. The total results of A2 were lower than those of A1, i. e. 60%, and 71%, but this difference was due largely to the falling off for a while on No. 1 compartment.

Two Seconds Delay.—The trials in which there were two seconds delay between turning off of the light stimulus and the release, were all of the A1 type and were not continued long because the results showed that so short a delay had no effect on the correct reactions of the dog. Thirty-three of these trials were given between October 25th and November 19th, 1913, with twenty correct, or a percentage of sixty-one. A slight improvement was noted toward the end of the series, the last ten trials giving an average of 70% of correct reactions. These results were so nearly identical with those obtained with "Light Out at Release" that it was decided to make five seconds the minimum delay length in later experiments.

Five Seconds Delay. A1—One hundred and thirty-eight trials of the type A1 were given between October 22nd and December 1st, 1913. Of these ninety-two, or a percentage of sixty-six and two thirds, resulted in correct reactions. The factor of attention cut into the number of correct reactions, but the results show that the dog could discriminate the three compartments with practical certainty after a delay period of five seconds. The best series were those on October 27th, 1913, when twenty trials showed seventeen correct reactions, the last ten being perfect, and on November 10th, 1913, when fifteen trials showed 82% successes. Here also the last ten trials were correct.

A2—To obviate olfactory stimulus, two series of ten trials each, were given January 7th and 12th, 1914, in which the food was given to the dog after successfully completing the reaction. The first ten trials gave eight correct results and the second ten, five correct reactions. The average of 65% was so near that of the average of experiments of the A1 type that the indications of the use of olfactory cues were negative. The question of rhythm could be applied here, but no special checks were given. However, checks on the simpler type of "Light Out at Release" coming immediately after the close of these series showed no evidence of rhythmic choice. There was no reason to think that the dog adopted a rhythmic choice for a five second delay, when he did not for a shorter delay period that was being used in experiments carried on at the same time.

Ten Seconds Delay. A1—On October 27th, 1913, the series of trials with ten seconds delays under the type A1 were begun and extended to November 19th, 1913. The results of fifty trials showed only twenty-five successful reactions. This was evidently owing to the breaking down of the cues to the middle compartment, and a return to a similar type of problem was suggested. Accordingly the "Two Light" experiments were employed again to determine what length of delay might be obtained, with a view of applying the resulting training when a return of the "Three Light" experiment should be made.

A2—On January 7th, 1914, the ten second delay was again tried under the type A2, and the benefit of training on the "Two Light" tests became apparent. The first fifteen trials showed ten correct reactions, of which the first five were successful. In the next series of ten trials on January 10th, 1914, all were successful. The 80% average showed that the dog had attained practical mastery of the problem. The likelihood of rhythmic choice of compartments was prevented by choosing beforehand a certain order and following it closely. No simple rhythm could follow the picked succession, viz., 1, 2, 1, 3, 2, 2, 3, 1, 1, 3, 2, 3, 3, 2, 1.

Longer Delays.—Scattered series of trials of fifteen seconds delay, or longer, were made on days when the dogs' attention was very keen. On November 15th, 1913, five trials, Condition A1, with a fifteen second delay were all successful. On January 9th, 1914, ten trials with twenty seconds delay, Condition A2, were all successful. As the interest of the experimenters was along the line of further reducing the possible cues by which the association might be made, no further trials were given to demonstrate the mastery of longer periods of delay with such complexity of cues as Condition A1 and A2. However, from the ease with which the dog made the reactions, the experimenters were satisfied that with the same degree of attention as then displayed, further trials would only definitely show that the dog could retain his cues to reaction over a delay period of at least twenty seconds. As later trials will show, this assumption was not unfounded.

Condition "B."—Condition "B" means that the results here were made under conditions in which the release box faced the food compartments only during the stimulus and delay periods. The dog could not see the operator adjust the lights and so could not gain any possible position cue to the right compartment from his movements. Also the possibility of smelling the food was removed, for in all the "B" trials, the food was thrown to the animal after the successful completion of the reaction.

B2—The results here embraced the following series:

Light out at release	92 trials	44 correct	48%
Five seconds delay	80 trials	48 correct	60%
Ten seconds delay	25 trials	11 correct	44%
Fifteen seconds delay	14 trials	6 correct	45%

These results are not high but nearly all well above a chance percentage of successful reactions. The averages obtained were about the same as those of the same length of delay in the "A" types and the differences are too slight to be explained on any ground, but that of the attention of the dog, which varied with his physical and pyschical state. It was noticed that the middle compartment seemed to be discriminated more correctly than the other two, so check experiments were given in the "Out at Release" to see if this was really so. On No. 1 compartment, seventeen trials gave six correct reactions, or a percentage of 35. Compartment No. 2 gave seven out of ten, or an average of 70%, while No. 3 compartment showed only thirteen out of forty-five trials correct. This last result confirmed the conclusion that the cues to No. 3 were weak and that more training was necessary before correct discrimination could be shown.

Close observation of the behavior of the dog showed that he avoided the right side of the room on which No. 3 compartment was situated because of the intensity of the light. Finally he refused to go there entirely. Of forty-five trials he reacted correctly in only thirteen, or a percentage of twenty-nine. In the last third of the trials he even refused to leave the release box, refusing twelve of the last fifteen chances. A dimmer light was put in No. 3 compartment and the next day the dog, working up from a "Light Constant" position gave 44%, or twelve correct out of twenty-seven trials. This was as good an average as he had before reached. With ten seconds delay he gave 44%, or eleven correct out of twenty-five trials. In both series the dog still avoided No. 3 compartment so another check of ten trials was given on it, with none of them successful. In the last three trials the dog refused to leave the box. When a trial on No. 1 was given, however, he left the box and reacted correctly. Further trials, with only compartments No. 1 and No. 3 in use, overcame the aversion to No. 3 and when the three compartments were again used, the reactions were as good towards it as towards either of the other two, viz.

Five seconds delay	67 trials	46 correct	68-3/5%
No. 1	16 trials	12 correct	75%
No. 2	27 trials	18 correct	66-2/3%
No. 3	24 trials	16 correct	66-2/3%

Condition "C." The experiments listed under condition "C" were those in which the release box was turned away from the food compartments during the delay period and in which the dog did not see the operator until after the entire reaction had been completed. The release box was turned facing the compartments just soon enough to release the dog at the end of the delay period and not allow him any time to see the compartments before he was forced to make his choice. This change of conditions was an attempt to forstall the probable chance that the dog was guided by orientation cues to a great extent and was not using memory cues. If so, and this orientation should be changed during the delay period, then he would not be able to react correctly. A minor point in regard to the effect of keeping the problem always before the dog during the delay period was also involved in the change. If this had had any effect of affording a cue to reaction, the turning of the box destroyed its function. The introducing of a new field of view during the delay that was entirely foreign to the problem was apt to have a distracting effect on the dog's attention and thus tend to drive away the memory of the proper association. The records from these experiments were taken as satisfactory proof of the question as to whether the belief in such a probability was well founded. The release box had to be turned rather quickly in order to disturb the orientation of the dog *within* the box, as well as to disturb the entire orientation toward the food compartments. The preceding observations had borne out the statement of Hunter (13), that the dog showed orientation only by the movement of the head, and that causing the head to change its position would effectively destroy the dog's orientation. The sharp turn of the release box affected this disturbance of orientation, for the dog was forced to move the whole body in order to maintain his equilibrium. Under these new conditions, no evidence was seen of the tense, eager waiting that the dog had formerly displayed during the delay period, and the dog even used the time for scratching at fleas, and during the longer delays would close his eyes and apparently take little cat-naps while waiting. As soon, however, as he felt the release box return to the normal position, he was wide awake and eager, and hurried out of the door before it was fairly opened.

The experiments are recorded under the two heads "C1" and "C2" differing as before, in the manner of placing the food reward. The records began with the minimal delay of five seconds after a five seconds light stimulus.

Five Seconds Delay. C1—Only fifteen trials of this type were given, as evidence of a breakdown on No. 3 compartment appeared and a return to an "A" type of experiment was necessitated. The results of the fifteen trials was nine correct reactions. Of the five trials on No. 3 compartment, all were wrong. Only one of the five trials on No. 2 compartment was a wrong reaction. At this time, November 4th-8th, 1913, the aversion or loss of cues to No. 3 was noticed in all the sets of experiments then going on, and so the series of check trials described under condition "A" was given. After this series the dog regained his cues to No. 3 and went there as readily as to any other compartment.

Ten Seconds Delay. C2—On January 22nd, 1914, the "C2" experiments were taken up again after the dog had had a month and a half's training in all the conditions including "AD" and

"CD" on two compartments. Resumption of the "A" and "B" types with the three compartments had been so successful that a ten second delay was used at once on beginning the "C" trials. Here the food was thrown to the dog after completion of the correct reaction. The records obtained here show perhaps better than any others the learning curve, beginning very low and rising gradually to a practical mastery of the problem. The first twenty-five trials were very poor, only seven being correct. Of the errors, eight were because the dog refused to leave the box at the release. The dog gave no attention at all to the stimulus and when he left the box on release it was more a result of training than a reaction to a formed association. The inattention was due to the lack of hunger.

On January 24th, 1914, the dog was more alert, and tried to solve the problem but was not very successful. Out of sixty trials only twenty-seven or 45% of them were correct. This low result was due to the dog's inability to retain the associations for the discrimination of the three compartments over a delay period of ten seconds, and was not due to inattention to the stimulus.

On January 27th, 1914, twenty-five trials were given and the benefit of the previous training began to be evident, for seventeen or 68% of the trials were successful. The last nine trials were all successful.

The record of the twenty-one trials given on January 28th, 1914, were not so successful as those of the preceding day, having only eleven or 52% of the trials as correct reactions. The falling off was due to the indifference of the dog to the stimulus in the last ten trials. The average for the first eleven trials was 73% and for the last ten only 30%.

February 16th, 1914 was the next day on which the "ten second delay" trials were given, and of eighteen trials sixteen or 88% of them were correct. The last twelve trials showed perfect reactions. The records made on this date were the best obtained on the ten second delay problem, for fifteen trials on February 10th, 1914 showed eleven, or 83% of them correct.

The learning curve obtained in these trials with the ten second delay shows the growth of the power of carrying the associations over the delay period the most perfectly of any of the delay series. All show the same general results, but none show such a smooth curve. A short summary of the data may make this growth more apparent.

"Five Seconds Light—Ten Seconds Delay."

1/22/14	25 trials	8 correct	27%	Falling off on
1/24/14	60 trials	57 correct	45%	Falling off on No. 3
1/27/14	25 trials	17 correct	68%	Last nine correct.
1/28/14	21 trials	11 correct	52%	Refused to act seven of last eight trials.
2/ 6/14	18 trials	16 correct	88%	Last twelve correct.
2/10/14	15 trials	11 correct	73%	Trials five to eleven correct.

That the low records at the beginning of the experiment were not due to the breaking down of orientation cues, is seen by comparison of the records for trials of "Ten Seconds Delay" condition "A2" given at the same time.

On January 17th, 1914 a series of thirty "A2" trials showed only 33-1/3% or "Chance" percentage of trials correctly completed. Of the "C" trials, a series of twenty-five on January 22nd, 1914 showed only six or 27% of them as correct reactions. This shows that the lack of attention to the stimulus was the cause of the failures and that attention and training must be gained before higher results could be obtained, as the results show there was a parallel rise of the learning curve in the problem given in both the "A" and "C" conditions. The added training and the better attention given to the stimulus gained a practical mastery of the problem at the same relative time, i. e. February 5th, 1914 for trials of Condition "A2" and February 6th, 1914 for trial of Condition "C2." The daily record of January 27th, 1914 showed very nicely the typical day's work with an advancement coming at the end of the series of trials. Many of the daily records showed a higher average of results for a single series, but this example is given because it shows the growth from a very low beginning to a high ending.

Stimulus at Compartment	3	3	3	2	1	1	1	2	3	3	2	1	3	3	3	3	3	2	1	3	2	2	1	2	3	25 Trials
Response at Compartment	1	2	3	2	0	2	1	2	2	3	3	1	2	0	0	2	3	2	1	3	2	2	1	2	3	Failures mostly on No. 3 Compartment
Correct			x	x			x	x		x	x	x	x				x	x	x	x	x	x	x	x	x	17 Correct—68%

The rhythm for these trials was very irregular, being selected beforehand and only varied by keeping the dog at a compartment till he had gone to it correctly. Then the regular schedule was resumed. This method seemed preferable to that of keeping to the fixed schedule strictly, as it gave the dog extra practise on the compartments to which his cues seemed to be weakened.

Fifteen Seconds Delay. C2—The fifteen seconds delay type of the experiments given under condition "C2" extended over the period between January 17th, 1914 and February 12th, 1914. In all there were sixty-seven trials, and of these forty-four, or 66-2/3%, were successful reactions. The last twenty-two trials showed much improvement and gave 85% of them as correct reactions. The highest record reached on any individual day were those on February 6th and February 12th, 1914, when nine out of ten and twelve out of fourteen trials were correct. The results of the earlier series were lower and showed a decreased learning power instead of an increased one as a result of training. This decrease was due to the apparent failing of the cues to No. 3 compartment. This failing was noted in all the types of experiments going on at this time, and a special check series of twenty trials on No. 3 compartment was given to restore these cues. After regaining the cues, the records given above as the highest obtained, were the results. The reactions to the other two compartments became more accurate, due to the training, as the records show, for while at first the errors on No. 3 were only 40% of the total number of errors, they were 71% at the end, just before the check experiments were given, and the total number of errors increased only 7%. As the increase of errors on No. 3 was 74%, the difference can only mean that there was a strong decrease in the errors on the other two compartments. The summarized results for "Fifteen Seconds Delay"—"C2," are as follows:

1/17/14	17 trials	10 correct	57%	Errors equal on No. 1, No. 2, and No. 3.
1/18/14	14 trials	8 correct	57%	Falling off on No. 3.
1/20/14	14 trials	8 correct	57%	6 of the errors on No. 3.
2/6/14	10 trials	9 correct	90%	Last five correct—Error on No. 3.
2/12/14	14 trials	12 correct	83%	Last five correct—Errors on No. 3.

The usual methods of avoiding possibilities of rhythm were used, with the modification of keeping up the trials on one compartment until a successful reaction to it was obtained. The method of the experiment eliminated all possible olfactory orientation or position cues.

Delays Longer Than Fifteen Seconds. The records quoted above were the highest and best results obtained, for problems of the "C" types that are complete enough to be the basis of claims of mastery. However, shorter series of the longer delay periods were given, with the view of discovering as to whether the cues used could bridge delay periods of longer duration than ten seconds. On February 10th, 1914, a series of ten trials of "Twenty Seconds Delay" "C3" were given, with 90% of the reactions correct. In the case of the error, an interesting sidelight was noticed: The dog did not see the stimulus, to the knowledge of the observer who had watched the animal carefully, but seemed intent on rubbing his nose with his paw. On release he automatically hurried out until in front of the compartment toward which he had faced at release. Just at the entrance he seemed to realize that a choice was to be made and paused suddenly, then he slowly walked to each entrance in turn and looked up at the light bulb as if seeking for a cue. Then he gave up and returned to the release box without making an attempt to enter any of the compartments. What the mental attitude of the dog may have been, the writer leaves to some experienced animal psychologist to explain. On the same day, the dog gave four out of five reactions correctly on "Five Seconds Light and Thirty Seconds Delay," which on February 12th, 1914 he reacted correctly in nine out of eleven trials of the same length of delay for a percentage of eighty-three. Five trials on the fourteenth were all correct and of the whole number of thirty-one trials, twenty-seven, or 87% were successful reactions. These trials, though rather few in number showed such a constancy in the per cent of successes that the experimenters believed that strong evidence of mastery of the problem was presented.

Two series of trials of "Forty-five Seconds Delay" were given on February 14th and 16th, 1914. Ten trials on the first day showed only five correct responses and ten trials on the second showed but six correct. Of the nine errors, five were for No. 2

compartment. Only three of the light trials on No. 2 obtained correct reactions. This seemed to indicate that the cues to No. 2 were almost lost. In the "Two Light" experiments (discussed in the next section) No. 1 and No. 3 compartments had been discriminated after a delay period of much longer duration, but here the cues to each of the three compartments did not seem to be localized well enough to be differentiated after they had had the chance to become indistinct during the delay period of forty-five seconds. The cues to No. 2 had so lost their individuality during this long delay that the dog could not differentiate them from those of No. 1 and No. 3 in even a "chance" number of attempts. As a desire was felt to increase the number of discriminated objects, and not to increase the delay period on the three compartments, no more trials were given on this type of experiment.

"*Two Light Experiments.*"

By November 19th, 1913, the experimenters saw that the dog had not received proper training to do well at the three light problems, and that success there would mean spending much longer periods of training on each phase than was at their command, so it was decided that a thorough training on the two lights would gain time and also give a test for comparison with the results of Hunter (13) on the length of delay possible to obtain with two lights. Another cause for the adoption of the two light method was the fact that the trials so far, had shown that the cues to No. 2 compartment were very weak and could easily be dropped out. The method used in the "Two Light" trials were slightly different from those in the "Three Light" types. From the beginning, the stimulus was retained at each bowl after a mistake until the dog made the correct response. He was also kept at each type until each series showed five successive correct reactions, before the difficulty of the problem was increased. These methods, it was hoped, would bring the association between stimulus and reaction to the correct compartment, much more strongly into mind. Previous training had failed to strongly set up this association. After the two light association was thoroughly learned, the addition of a third compartment would be much simpler than trying to teach association to all three compartments at once.

The results were extremely successful from the very first, and showed practical mastery of all the phases of the "A," "B" and "C" types immediately.

" Two Light " trials were given in condition "A" of the following types, " Light Constant," " Light Out at Release," " Two Seconds Delay," " Five Seconds Delay," " Ten Seconds Delay," " Fifteen Seconds Delay" and " Sixty Seconds Delay" with the following results. All these results showed a high percentage except those on "Five Seconds Light, Fifteen Seconds Delay" when the dog was very fatigued and gave but poor attention at the beginning of the trials. In all cases each series was continued until at least five successive correct trials were made.

A short summary of the trials is as follows:

Light constant	8 trials	8 correct	100%
Light out at release	5 trials	5 correct	100%
2″ delay	10 trials	9 correct	90%
2″ delay	34 trials	32 correct	97%
10″ delay	12 trials	11 correct	92%
15″ delay	16 trials	11 correct	69%
60″ delay	10 trials	8 correct	80%

The results for condition "B" which follows in summary, were not so favorable, yet results beyond that of 50% of chance were obtained in all but three series of ten each on 11/17/13, 11/19/13 and 12/16/13. On each of these three days the dog showed symptoms of indigestion. These poor days pulled down the general averages for the types in which they occur, but the other records of each corresponding type were high enough to balance up and give a safe margin over the 50% of " Chance."

5″ delay	26 trials	18 correct	69%
10″ delay	22 trials	14 correct	64%
15″ delay	18 trials	11 correct	61%
30″ delay	15 trials	10 correct	66-2/3%
60″ delay	27 trials	18 correct	66-2/3%
120″ delay	11 trials	8 correct	72%

The results for condition "C" were better than those of either condition "A" or " B," as a much better practical mastery of each type was shown

5″ delay	35 trials	30 correct	86%
10″ delay	30 trials	28 correct	93-1/3%
15″ delay	15 trials	15 correct	100%
20″ delay	10 trials	10 correct	100%
30″ delay	38 trials	27 correct	70%
60″ delay	13 trials	7 correct	54%

The results on "Sixty Seconds Delay" were but little better than chance, because of an aversion to No. 3 compartment, due primarily to a too brilliant light. When this was replaced by a dimmer light on shorter delay trials No. 3 compartment was chosen as well as No. 1.

The success of these series of trials led the experimenters to attempt various types of diversion during the delay period, first in condition "A" and then in "C." These experiments were recorded under the heads of condition "AD" and "CD" respectively.

Condition "AD"

In condition "AD," meat was held in sight of the dog during the delay and the experimenters talked and whistled to him in order to keep his attention away from the problem before him. For the first five of the seventeen trials given, the dog did not do very well, and seemed to have lost his cues, but soon regained them and the diversion seemed to have no effect on correctness of reactions, for the last nine reactions were all correctly and unhesitatingly made.

The exact record will show the effects, the best of any way.

Stimulus at Compartment	1	3	1	1	1	1	3	3	3	3	1	3	1	3	3	1	3	17 Trials
Reaction at Compartment	1	3	3	3	3	1	3	0	3	3	1	3	1	3	3	1	3	4 Errors
Correct	x	x				x	x		x	x	x	x	x	x	x	x	x	13 Correct

Condition " CD."

In this condition diverting stimuli were given during the delay period of condition " C." These stimuli consisted (1) of the sight of operator walking in front of release box, (2) the sound of the operator's voice calling the dog, and (3) a whistle call with a piece of meat hung before the door. The object was to find out if such stimuli could or would cause the dog to forget his cues to the food compartments. The dog answered each of these diversions by giving his whole attention to them, often by snapping at the meat through the screen or by getting up and turning around when called and not released.

These series of trials of this condition "CD" were given on December 8th, 1913 and December 15th, 1913 and concluded the trials on the "Two Bowl" problems. The first series on December 8th, 1913 was of Ten Seconds Delay with 100% results. Five trials of One Hundred and Twenty Seconds Delay also showed perfect reaction with no evidence of the effect of the diverting stimuli. A series of light trials on Sixty Seconds Delay followed and showed 75% of the reactions correct, the last five being perfect. As there seemed to be no sign of the dog losing his cue to either of the compartments, a sudden jump was made to a delay of five minutes, and a series of five trials showed all as perfect reactions. There was no hesitation displayed in the dog's choice of compartments, nor did there seem to be any sign of an hindering influence, due to the diverting stimuli introduced during the delay. In order to ascertain whether these results were chance, or whether they showed the power of retaining a cue powerful enough to bridge the five minute gap, and then discriminate perfectly between the two bowls, another series of trials on these long delays was held on December 15th, 1913. As usual the dog was kept at a compartment until he succeeded, and no set of trials was given up until five in succession had been correct. Beginning with a two minute delay seven trials resulted in the last six being correct (or a percentage of eighty-six.) With a three minute delay, five trials were all correct. Then a five minute delay showed seven successive correct trials. Then the next trial was unsuccessful for the dog seemed to show fatigue and during the delay period dozed in his box and did not pay attention to the attempts of the operators to attract his attention. On release he went out slowly and wandered around and finally reached the food compartments and entered the wrong one. Because of the loss of attention, no more trials were given.

No attempts at longer delays were made as no further object seemed to justify their continuance. During all these trials of the "Two Light" type, the usual method of avoiding rhythmic succession of trials was used, as well as the system of throwing the food to the dog after a correct reaction had been made. This latter method avoided the possibility of the use of the olfactory stimulus as a cue to reaction. The time gaps already bridged were too long for any sensory after image to have been retained and hence diversion stimuli would have no lasting effects on the cues by which the dog retained the association. No attempt was made to find out definitely what those cues were. The above results show that the power of discrimination of two objects, fairly well separated (six feet), was within the untrained capacity of the dog, and that the responses given after the delay periods were the result of training. This training enabled the retention of these powers of discrimination, necessarily through some process, presumably memory. The negative results gained by diverting stimuli during the delay period, show that mere physical clues or sensory after images could not suffice as cue retainers, but that there must be a definite mental process involved, that may be called "Memory Association." Since an association of stimulus and food compartment was necessary, and since correct reaction demanded that such an association be retained by some mental process, the "C" type of problems having shown the absence of the physical cues, therefore, as psychologists agree that sensory after images could not bridge a gap nearly as wide as the five minute one that this dog has done, the process must be one that involves the memory.

"Four Light Experiments."

In the middle of February, 1914, it was suggested that the problem be made more difficult by increasing the number of compartments, the cues to which must be remembered, rather than by the continued increasing of the length of delay periods over which the association cues must bridge. So to increase the number of compartments, No. 4 was added, but not on the same ground level as the other three. The food was obtained only by going up to the compartment on a board, eight feet long and having a 30 degree pitch. The light stimulus was however at the same height from the floor as in the other three compartments. In order to quickly teach the dog the habit of going to No. 4 compartment, and to climb the inclined board without fear, the operators "put" the dog through the reaction. That is, one held a piece of meat at the top of the incline and called the dog. The other experimenter led and pushed the dog up the incline. After four trials, in which he was helped, the dog ran freely up the incline when called, without showing any signs of fear. Then three trials were given in which both light and voice were used as stimuli and the dog released immediately from the release box. Each one of these trials was successful. Next, three trials were given in which the light alone was the stimulus, it being removed as the dog was released. These three trials were all correct. From this point the regular procedure of the experiments was taken up again. Five trials of "Five Seconds Delay," position "C3" were given on February 6th, 1914 and all were correctly reacted to. On the same day series in "Ten Seconds," "Fifteen Seconds" and "Thirty Seconds Delay" were given, all in condition "C3." Of the ten seconds delay, five trials were all correct, as also were five trials of fifteen seconds delay. On February 17th, 1914 a series of nine trials of twenty seconds delay was given, of which seven or 76-2/3% were correct. The last five trials were correct. On four different days, series of trials of "Thirty Seconds Delay" were given and the record for the last day showed very great improvement, over those of the preceding ones. In the last three series, five successive correct reactions were obtained before any longer delay was attempted. The first day the dog broke down entirely on No. 3 compartment and a shorter delay had to be used. The records for the four days are as follows:

2/16/14	9 trials	4 correct	45%	
2/17/14	14 trials	9 correct	64%	Last 5 trials 100%
2/19/14	6 trials	5 correct	83%	Last 5 trials 100%
2/21/14	9 trials	8 correct	89%	Last 5 trials 100%

Encouraged by these results, on February 17th, 19th and 21st, series of trials on "Forty-five Seconds Delay" were given. On February 17th, five trials were all correct. On February 19th, the last five of seven trials were correct, giving a percentage of seventy-one. On February 21st, twelve trials were given and of these trials 2, 4, 5, 6, 7, 8, 9, 11 and 12 were correct reactions giving a percentage of seventy-five. This series gave six successive correct reactions in trials four to nine. On the 19th, nine trials on a one minute delay were given and 66-2/3% of them were correct reactions. The last five were 100% correct.

These experiments showed that the dog was able to accurately discriminate the stimulus from four compartments, that were only two feet apart, and that he was able to retain the cues to this discrimination over a delay period of at least one minute, without showing signs of a break-down in the cues to any one of the compartments.

RESUMÉ OF RESULTS

The first conclusion drawn from the records was that perfect mastery could not be gained because of the vacillating factor of attention. This factor was high enough to give practical mastery of the problems, but failed to give perfect mastery. Sheepish behavior was apparent whenever the dog did not feel exactly right, or when he made several unsuccessful trials in succession. After such a period of failure the dog would hesitate for fear of more mistakes, and in doing so would fail to get the stimulus and make the association. This sheepish action was the cause of a large percentage of the failures to make the correct reaction. It was found, however, that allowing the dog to return to the release box at his own will did away, to a great extent, with sheepish actions. If the dog was always called back, after several failures he would become discouraged and give up trying. If he was left to his own devices, after several failures he would lie down and rest for perhaps two minutes and then return of his own free will and try over again with his attention and interest as alert as ever. No sign of sheepish behavior appeared then.

The records also show the contrast between the different parts of the experiments, i. e. the routine of leaving the release box when the door was opened, and that of making the correct reaction to the stimulus given. Often when he refused, through discouragement, to make a choice of compartments, he went through the routine part without a hitch. It was found, however, that extreme discouragement affected even the routine part and the dog refused to even leave the release box on the opening of the door. On command the routine part would be resumed, but correct reactions, or even attempts at choosing did not follow such commands. Hunger also caused attention to vary. When very hungry, the dog was in good attention and gave good results, and vice-versa.

Another interesting factor in regard to the preference for any certain compartment was found in the records. A summary will give a better idea of this preference than can the mere description above.

TRAINING TRIALS

Condition "A"—Three Lights	101 trials	85 correct	85%
No. 1	32 trials	30 correct	94%
No. 2	34 trials	26 correct	76%
No. 3	35 trials	29 correct	83%
Condition "B"—Three Lights	81 trials	43 correct	53%
No. 1	20 trials	12 correct	60%
No. 2	33 trials	15 correct	45%
No. 3	28 trials	16 correct	57%

DELAY TRIALS

"Three Lights"

Condition "A"	381 trials	249 correct	64%
No. 1	128 trials	87 correct	68%
No. 2	132 trials	77 correct	51%
No. 3	121 trials	85 correct	71%
Condition "B"	211 trials	118 correct	56%
No. 1	68 trials	36 correct	53%
No. 2	74 trials	46 correct	62%
No. 3	69 trials	36 correct	47%
Condition "C"	310 trials	193 correct	64%
No. 1	103 trials	60 correct	59%
No. 2	107 trials	73 correct	68%
No. 3	100 trials	60 correct	60%

"Two Lights"

Condition "A"	95 trials	85 correct	89%
No. 1	46 trials	44 correct	95%
No. 3	49 trials	41 correct	84%

Condition “B”		119 trials	78 correct	65%
	No. 1	53 trials	39 correct	73%
	No. 3	66 trials	39 correct	59%
Condition “C”		141 trials	108 correct	76%
	No. 1	69 trials	63 correct	91%
	No. 3	72 trials	45 correct	62%
Condition “AD”		17 trials	13 correct	76%
	No. 1	8 trials	5 correct	62%
	No. 2	9 trials	8 correct	90%
Condition “CD”		51 trials	46 correct	90%
	No. 1	30 trials	26 correct	87%
	No. 3	21 trials	20 correct	95%
“Four Lights”				
Condition “C3”		90 trials	71 correct	79%
	No. 1	24 trials	21 correct	88%
	No. 2	23 trials	13 correct	57%
	No. 3	20 trials	16 correct	60%
	No. 4	23 trials	21 correct	91%

The above summary shows that the associations toward the different compartments varied considerably. Any two of the compartments would have perfect associations while the other would be neglected. The next day the result would be vice-versa, and the neglected one would become the compartment best associated. Thus, while one day's record would show more trials at one compartment than at another, the whole series of trials showed about an equal number of trials on each one. This variation might be construed as an argument against the possession of reason by the dog. If the light always meant the compartment that contained the food in the three light trial, why did not the dog go to the light when it was placed over a fourth compartment? This presupposes that the dog had mastered the association between the light and reaction necessary to obtain the food and gave good results in the three light experiments. The fact is that he did not, of his own accord, make the new association. Evidently if reason was working here as a factor in association, seeing the light anywhere over a dish would cause a motor impulse to go to it. But, as it did not, it is evident that reason was not the cause of the formations of the associations. That habit is the cause of association being formed is shown by the fact that after being taught to go to the extra compartment by special training, and the habit of going there firmly fixed, a series of trials including the four compartments showed that the association of stimulus and response is just as perfect for the fourth, or new one, as for any of the former three. An argument that would seem to point in just the opposite direction, tending to support the idea of the functioning of reason, is given in the actions of the dog in cases of wavering. In some cases it was noticed that the dog would start for one compartment, and then swerve, often very sharply to another, sometimes wrong and more often right. Was it reason that made the dog believe that he had made a wrong choice and caused him to change his selection, or was it the motor energy in his muscles that forced him out in whichever direction he happened to be facing when released ? Was he unable to set up the motor reaction connected with going to the compartment for which he had made the association between the movement to get the food and the light stimulus until the first momentum was over ? To the experimenters it seemed that the use of reason is the most feasible solution, but the psychology of such a question is left to men more trained in that line than the writer. A still more striking and less easily explained action, was manifested when on getting almost to some compartment, the dog would stop, hesitate and then go ahead or choose another compartment, sometimes wrong, sometimes right. A few times the dog gave up entirely and returned to the release box without even trying. Did the dog forget his cues, and realizing it, attempt to reason out the correct association, and on failing to do so, would return to the release box; or did he fail to get the cue and form the association, and, from habit, rushed out when the release box was opened ? That is another question for the trained psychologist to answer.

In condition "A1, Three Lights" it was seen that No. 2 compartment was discriminated only 88% as well as No. 1 and No. 3. Thus it shows that while it was easy to discriminate two widely separated compartments, the addition of the one in the middle made it much more difficult to keep its cues separate from those of the other two.

In condition "B, Three Lights," it is noticed that compartments No. 2 and No. 3 are discriminated equally well and that No. 1 is the one not so well discriminated. This failure on No. 1 was due to a long period of fear of the light there, as the dog had burnt his nose on it at the beginning of the "B" trials. The equal discrimination of No. 2 and No. 3 shows that the training on No. 2 received in condition "A" had borne fruit. In condition "C—Three Lights," compartment No. 2 was discriminated 25% better than either No. 3 or No. 1 .In both compartments, the dog had lost the cues to their discrimination several times and had been given special training on them. The previous training on No. 2 was shown in the results of the trials. This trial in the power of discriminating No. 2 from the others is a proof of the benefit of special training in habit formation, for the habit of going there on stimulus, had become so firmly fixed that errors became steadily less, and passed in strength, the habits that caused him to go to the others on stimulus.

In the "Two Light" experiments, condition "A," "B" and "C" show better discrimination of No. 1 than of No. 3 by about 18%. This favoritism was due to the fact that the light in No. 3 was too bright, and after changing to a dimmer one, it took over a week before the fear of No. 3 was sufficiently overcome to allow the dog to enter freely whenever the stimulus was given there. As the "Two Light" trials took place in a period of only two or three weeks, this trouble with No. 3 materially affected the general results of a large number of trials.

In the trials of condition "AD" and "CD" that took place in the period between December 12th and 15th, 1913, the results were reversed and No. 1 was the one that was not so well discriminated. In condition "AD" the difference was too small to be noticed but in condition "CD" a difference of 11% was found. This difference was due to the fact mentioned in the "Three Light" types, i. e. that the dog had burned his nose on December 15th, and hesitated to go to No. 1 after that. This set of experiments was concluded on this day and the "Three Light" type resumed, but the fear of No. 1, however, was carried over and effected the records there. In all these trials, care was taken that, while rhythmic successions of choices of compartments was avoided as much as possible, the number of chances was approximately the same for each compartment. The usual result was that the compartment that was discriminated the least accurately received a few more trials than the others.

Another point gained from the records is that the reaction comes, not from seeing a light over a compartment and then going there when released, but from the fact that when a light was shown over a *certain* compartment to which he had been *trained* to go on stimulus, the dog went as a matter of habit. Hence, a light over a compartment to which he was not in the habit of going did not set up in the dog's mind the association of "Light—Movement to Light—Food" or for the incitations to the motor response that comes on release, that the training to that compartment would. The dog must be in the *habit*, gained through thorough training, of going to an indicated compartment on release, or he would not pay any attention to it, but on release would go to one of those to which he was accustomed to go, even though he had received no stimulus to go there. The *mechanical* is stronger than the *reasoning* in this case. Addition to the number of compartments means that the dog must have special training on the new one before it can become one of the *several* to which he is trained to go on release after a stimulus had been given.

In the "Three Light" problem, condition "A" shows that by course of long training a light stimulus can be made effective, and the dog can remember the cues formed and the associations set up by the light stimulus for the three compartnemts over a period of one minute delay between stimulus and release. The latter part of the records show that the olfactory stimulus to reaction was not an important factor, as the records were as good as when the presence of the food in the bowl gave the possibility of such stimulus.

The records from Condition "B" show that the sight of the operator placing the food in the bowl and operating the lights gave no cue that was necessary for correct reactions, as the general results of "B" are practically the same as those of "A" while the dog could not see the operator until after the entire reaction had been completed.

Condition "C" gives results of the effect of orientation and the retaining of the compartments in view during delay period, on the correctness of reaction. The records show that the dog did not use orientation cues at all, or that they were of such minor importance that they were dropped without effecting his reactions. The turning of the release box during the delay, thus destroying orientation to the food compartments and the disturbing effect that a new set of objects before the sight would have, did not give records any lower than those of condition "A." In fact the records for the discrimination of the three lights was better than those of "A," and the delay period reached was of the same length in both conditions. From these results the experimenters claim that this dog should be classed with the group in which Hunter (13) places his raccoons, i. e. the group that does not depend on the retaining of orientation to give them the proper cues to obtain correct reaction.

The "Two Light" experiments show that the discrimination of two compartments is easily within the native capacity of the dog, for without previous training, once the light association is formed, he gives 100% results. Perfect mastery is here shown for all the conditions for delay periods up to one minute for "A," two minutes for "B" and one minute for "C." Condition "CD" gives results that show that diverting stimuli, such as sound, sight, smell, etc., during the delay period of condition "C," do not materially affect the power of correct discrimination of the two compartments even though the cues must be retained over a period of as long as five minutes. In none of the delays of any condition, was there any sign of breaking down in the cues to any compartment, in the longest delay periods used. No attempt was made to find the limit of the time over which the dog could retain the association cues, in any of the problems.

The "Four Light" type was tried only in condition "C" and it was found that through the benefit of previous training, the dog, after a thorough training on the new compartment, could discriminate the four compartments up to a one minute delay as well as he had the three compartments. The first trials showed, however, that without definite training, and the formation of the habit of going to compartment No. 4 on stimulus, the dog paid absolutely no attention to the extra compartment, even though the light was constant, but went to one of the compartments to which he was accustomed to go on release.

SUMMARY OF RESULTS

(1) The habit of going to a compartment on release must be formed for each one separately before discrimination of from two to four can occur.

(2) The discrimination of four compartments was retained for at least a delay period of one minute, and of a fewer number of compartments, for a much longer period.

(3) The discrimination of two objects is within the native capacity of the dog.

(4) Visual and olfactory cues are not necessary for correct reaction.

(5) Signs of orientation may be prominent in the dog, but such cues were not important for the success of the delayed reaction in the animal experimented upon.

(6) Distractions of all kinds, visual, olfactory and auditory, or such as might arise from the natural behavior of the dog, may not necessarily interfere with successful delayed reactions.

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THE LOCALIZATION OF SOUND IN THE WHITE RAT

ALDA GRACE BARBER

From the Psychological Laboratory of the University of Texas

INTRODUCTION

The experiments presented in the following paper are intended as a preliminary study of the white rat's ability to localize sound. Meyer, Johnson, and Szymanski have also made preliminary studies, of less extent, however, upon other animals.

Meyer's[1] problem was to investigate (1) when and how localization appears in mammals and (2) the date of the appearance of the ability to localize correctly. As a result of his work he concludes that ability to localize depends upon: (1) mediate factors of vision, tactual sensations, habit and experience; (2) the immediate factor of hearing. Furthermore, he says true localization of the second type (hearing) is a function of binaural hearing, and mentions the factor of intensity. To test the above problems, he used humans and various species of animals: 47 human nurslings, 16 older children and 100 animals, 9 of which were less than a year old. With children he found that the difference between location of familiar and unfamiliar sounds seemed to be the essential factor. Six stages in the evolution of hearing were distinguishable in the human nurslings, comparable to the stages in the animals tested. The nurslings localized sounds as early as 7 weeks, although there was considerable variation. By the end of 6 months, however, practically all of them localized calls and noises. His work with animals falls into the following groups, a whistle furnishing the stimulus: (1) 2 dogs (5 days old) gave no reaction; (2) 1 Samali sheep (8 mo. old), 2 bears (6 mo.), attempted to localize sound but could not; (3) jaguar (5 mo.), elephant (5 mo.), leopard (13 mo.), panther-leopard, localized quickly and accurately; (4) older animals, including lions, tigers, panthers, hyenas, bears, elephants, antelopes, zebras, sheep, angora goat, various species of apes, land and water turtles, and 2 serpents, all localized with varying degrees of accuracy. If the animal oriented to the sound, Meyer presumed that it was endeavoring to localize. Meyer has made a table cataloguing the reactions of all these animals. No very definite conclusions were gained because of the roughness of the experimentation.

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[1] Meyer, Julius. Die Benutzung der Schalllokalisation zum Nachweis von Hördifferenzen; ihre Verwertung als Simulations-probe. *Monat. f. Ohrenhk.*, bd. 46, S. 1-15. 1912.

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Johnson [2] and Szymanski[3] have each made a few tests on the localization of sound by dogs. (The latter author has worked with cats also.) Johnson found that his four dogs learned to go to the source of sound after from 105 to 165 trials. Szymanski's animals failed after from 21 to 30 (?) trials. (S. attributes the failure to the small size of the experiment box which measured 9m. by 2.7 m. More probable causes are the small number of trials given and the pernicious position habits that developed. Johnson's box was differently constructed. He does not give its dimensions in detail, but I judge it to have been 24 ft. by 12 ft. The variations in size in the two boxes are thus negligible.) In each experiment, the stimulus consisted of a sound (a buzzer with J. and a bell with S.) which could be given in either of two positions in front of and to either side of the subject. (4 m. away in S's work, about 10 ft. in J's study.) No attempt was made in either case to increase the number of positions in which the sound might appear; and in Johnson's work little or no attempt was made to determine the cues used in securing the positive results.

In the present paper, I have been concerned primarily with the following problems: (1) How accurately will white rats localize sounds? (2) To what extent does intensity (both absolute and relative) determine the accuracy of response? The results throw additional light upon several supplementary problems: sensitivity to tone and the nature of the learning process. The study was made at the University of Texas during the session of 1913-14, under the supervision of Prof. W. S. Hunter.

Notes on Animals Used. Seven rats in all were used during the experiment. Rats No. 4 and No. 6 (male) were obtained from a dealer, January, 1913. They were approximately 6 weeks old at the time. Rats No. 19, No. 21 (males) and No. 20, No. 22, No. 23, (females), reared in this laboratory, were about 2 months old at the time the experiment was begun upon them (October, 1913.) No. 19 was dropped after the first preliminary trials upon methods of learning, because the method in which it had been trained was discontinued. The older rats were more deliberate in their movements than the younger.

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[2] Johnson, H. M. Audition and Habit Formation in the Dog. *Behav. Mon.* vol. 2, No. 3, 1913. pp. 46-51.

[3] Szymanski, J. S. Lernversuche bei Hunden und Katzen. *Pflüger's Archiv.*, bd. 152, 1913.

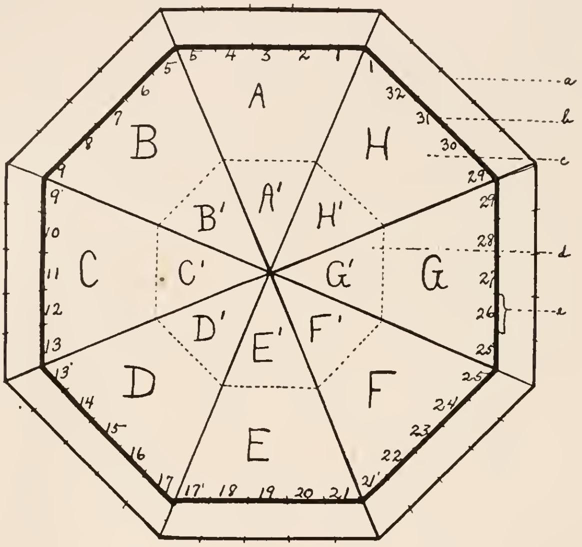

Fig. I.

FIGURE 1. Ground plan of apparatus. a, the stimulus board; b, the main apparatus box; c, outer triangle; d, inner triangle; e, section (approximately 2 ins. wide).

APPARATUS AND GENERAL METHOD

A. *Apparatus. Apparatus Box*—The apparatus in the present study consisted of an eight-sided box surrounded by a stimulus board. (See Figs. I & II.) All was made of light wood. The floor of the box was divided into isosceles triangles, lettered A, B, C, etc., in order to facilitate the noting of the positions of the rat. The sides of the box were divided into 32 sections all of which were approximately 2 inches wide with the exception of the corner ones which were from 3 to 4 inches wide. The stimulus board, 2-1/2 inches away from the main box, was fastened with wooden pegs to the table. This was done to prevent, as far as possible, the vibrations from passing through the wood between the point of sounding the stimulus and the point where the rat was standing. An opaque screen containing peep holes was erected about the apparatus in order to conceal the experimenter from the view of the rats. Measurements of the apparatus are as follows:

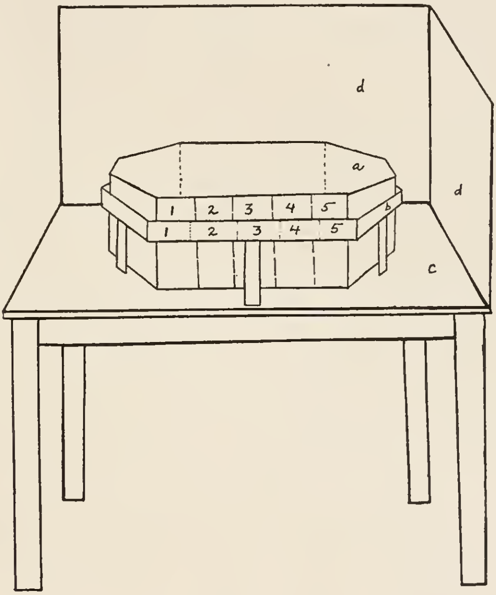

Fig. II.

FIGURE 2. Entire apparatus in perspective. a, main apparatus box; b, the stimulus board; c, the table; d, two sides of the screen behind which the experimenter stood.

Minimal width of box	22 in.
Length of sides	9-9 1/2 in.
Height of sides (inner)	7 7/8 in.
Distance of stimulus board from box	2 1/2 in.

Length of sides of stimulus board........10 1/2-11 1/2 in.
Width of stimulus board.................3 in.
Height of stimulus board from table......3 1/4 in.

The box had inevitable defects. (1) The fact that no release box was used allowed the rat to wander at will, getting the stimulus from different angles and positions. No constant standard head position could be gotten. (2) The reflection of the sound probably differed as the rat was nearer to the center or to the side of the box. Finally, (3) the sound might be diffused along the boards and thus prevent very accurate localization. However, in spite of these defects, the localizations were quickly and comparatively accurately made.

Various methods suggest themselves by which a relatively better control of the position of the rat with respect to the point of origin of the sound could be obtained. This of course is one important feature necessary for a comparison of animal experimentations with the human work in the localization of sound so far accomplished. Perhaps Pawlaw's salivary method would offer the best solution of the difficulty in tests with such animals as are suited to the method. The animal's head here could be fixed in a stationary position during the test. Presumably, after the association of a definite localization of the sound stimulus with food had been set up, the animal could be tested for the accuracy of change of localization in the different planes. This accuracy would be measured in respect to the quantity and quality of the flow. Monaural hearing should be tested here also.

Stimuli—The standard instrument for giving the stimulus (a tapping upon the stimulus board) was a medium weight chisel, the tapping of which had a predominant pitch between 256 d. v. and 512 d. v. This was determined by the use of Helmholtz resonators. Other stimulus sounds were made by: (1) tapping upon the stimulus board with the rubber of a lead pencil; (2) sounding a 256 d. v. tuning fork outside the screen; (3) giving the interrupted tone, 256 d. v., on an organ pipe; (4) hissing through the teeth; and (5) tapping with a lead pencil rubber upon the resonator box of a 256 d. v. fork. In giving this last stimulus, the resonator box was held in the hand outside the screen, and was tapped on the upper surface 1/4 of the distance from the opened to the closed end or half way between the open end and the fork. The intensity of the resulting sound was approximately the same as that made by the pencil (6 1/2 gr. weight) when dropped from the height of three inches upon the resonator at the same point used in the tapping. Here, as in the giving of the stimulus, the resonator was held in the hand. The intensity of the sound of the chisel was roughly the same, while the intensity of the tapping on the stimulus board with the lead pencil rubber was approximately equal to that caused by dropping the pencil above used on the resonator box from a height of one inch only.

The interrupted tone on the organ pipe was given for comparison with the interrupted noise produced by tapping. The stimulus from the fork and the hissing were comparable in that both were continuous in character. To the experimenter, the sound of the 256 d. v. fork seemed lower (undoubtedly due to the absence of overtones) than the tapping upon the 256 d. v. resonator box although both resounded to the Ut 3 Helmholtz resonator. The former was also decidedly more characterless.

B. *Description of Method*—The problem which the present experiment set the rat was the establishment of an association between the location of a sound (normally the tapping upon the stimulus board) and food. The experiment fell into three periods, which may be characterized as follows: (1) a period in which the rat was fed inside the box in order to accustom him to the apparatus and the experimenter; (2) a period of learning the association between tapping and getting food at the point of tapping; and (3) a control period in which the determining cues for the behavior were sought.

The procedure of each trial of the regular tests was as follows: (1) The position of the rat was noted at the moment of tapping. This included the direction of his head in respect to the section tapped as well as the absolute position of his body in the box. (2) The tapping signal was given on the stimulus board at the middle of a certain section. This tapping was *continued until the rat stood up at some section on the side of the box.* (3) The path of the rat from his starting point to the standing up point was noted. (4) Finally, the rat was *fed at the section tapped* to which he had to come if he had incorrectly localized elsewhere. No punishment was given if the rat did not react correctly, with the possible exception of the fact that he was consequently delayed a moment or two in being fed. As the rat was allowed only a nibble of the bread held over the side of the box, his zest for the work was not impaired during the 8 trials given per day. The records were noted in symbols, e. g. ← A′ tq B′ C D 16-14. Interpreted, this means that the rat was standing in the inner triangle, A′ (see fig. 1), headed away from the experimenter (←) when the signal was given, turned quickly (tq), went through triangles B′, C, D, standing up finally at section 16 in D, while the experimenter was at section 14.

The accuracy of localization is therefore 2. A regular series of presentations was used, in which each of the 32 sections of the box (see fig. 1) was given once every four days while each side of the box was given once every day. The order was as follows:

1st day	A	D	B	F	C	H	E	G
	1	16	8	21	10	30	19	27
2nd day	B	F	D	H	E	G	C	A
	6	23	15	32	17	26	9	4
3rd day	C	H	F	A	D	E	G	B
	12	29	22	2	13	18	28	7
4th day	D	A	C	H	F	B	G	E
	14	3	11	31	24	5	25	20

The capitals stand for the different sides of the box, and the numbers for the sections. Daily records were tabulated from the reactions of each rat by averaging the eight trials. The results are in terms of error, i. e., of accuracy of localization, denoting how many sections the rat missed the point of tapping. The reaction of the rat was considered completed when he stood up, and no further record was made for that test, regardless of subsequent behavior.

After the rat had been trained to a degree of accuracy which was reasonably constant from day to day, whereby the fact was established that the animal was localizing something, the next phase of the problem was considered, viz., was the reaction an auditory one. To establish this point controls were put in which eliminated kinaesthetic, olfactory and visual cues. Then, since the association still persisted, an effort was made to discover the auditory factors determining the accuracy of localization.

Several difficulties arise under the present plan of experimentation: (1) the rat, although alert and eager to respond, has to inhibit its own movements; (2) the tendency is to go in the direction headed (see fig. 3, below p. 300); (3) a reflex recoil is manifest at unwonted, unexpected noises; (4) flightiness or unstable attention is present; and (5) no punishment was given for incorrect responses, a condition which probably lessened accuracy and quickness of learning. These factors are all of importance in differentiating animal work from that done on humans. In addition one should note the difference in recording judgments in rats and humans, for the latter have only to indicate the point localized or to respond according to a familiarized chart, while the former have actually to go to the correct section.

EXPERIMENTAL SECTION

A. *The learning of the association*—The first question, i. e., whether the rat could localize the noise, was quickly answered, for an accuracy which was not improved in degree throughout the later part of the experiment was obtained within from 40 to 136 trials, i. e., in from 5 to 17 days. Table 1 shows the number of trials necessary for each rat to reach the standard reaction, which was considered achieved when the rat did not vary markedly in response for several days.

TABLE 1

Rat	6	20	4	21	22	23
No. trials on learning	136	40	136	64	48	48

An interesting observation in connection with this table is that rats 6, 4 and 21 were males, while rats 20, 22 and 23 were females. This difference in trials on learning may be due to chance, or to the fact that rats No. 6 and No. 4 were older than the others.

Records of the average daily error for rats No. 21, No. 22, and No. 23, for the first 15 days are in Table II.

TABLE II.

Rats	No. 21	No. 22	No. 23
1st day	3 2/7	6 6/8	6/4/8
2nd day	3 2/8	5	6 4/8
3rd day	3 2/8	4 3/8	4 5/8
4th day	4	4 5/8	4 5/8
5th day	1 4/8	1 5/8	6/8
6th day	1 5/8	3 6/8	5 3/8
7th day	2 5/8	1	1 2/8
8th day	3	7/8	1 7/8
9th day	1 4/8	2 2/8	1 2/8
10th day	2 7/8	1 6/8	4 6/8
11th day	1 (I)*	1 1/8 (I)*	2 2/8 (I)*
12th day	3 1/8 (II)*	2 3/8 (II)*	1 4/8 (II)*
13th day	1 5/8 (")*	3 1/8 (II)*	1 4/8 (II)*
14th day	2 4/8	6/8	1 5/8
15th day	6/8	1 5/8	1

* On these days controls were put in, but as they did not affect the rats' reactions the trials were considered standard.

The high average of No. 23 on the 10th day was doubtlessly caused by the noise in the laboratory resulting from an electric storm. One bad reaction of No. 22 on the 13th day, accounts for the high average, for the other reactions were up to standard. The average for No. 21 on the 12th day can be accounted for in a similar manner.

Throughout the tests the responses of the rats never became automatic. If a curve were plotted of these normal responses, it would show marked irregularities throughout its course. This contrasts with the regularity of the ordinary maze co-ordination in rodents—a motor or kinaesthetic habit.[4] Other investigators working with other problems have noted a similar irregularity in certain sensory habits. For example Vincent[5] in studying the ability of the white rat to learn the maze problem when olfactory and visual stimuli had been added and utilized secured results of this type. Watson[6] and Hunter[7] point out a similar lack of automaticity in the maze habits of birds. A thorough study of the factors governing these curve differences has not been made at the present time.

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[5] Vincent, S. B. Some Sensory Factors in the Maze. *Psych. Bull.*, vol. 10, p. 67, 1913.

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The length of the periods of learning the auditory localization in our problem are interesting when compared with an experiment on the effect of using food as the stimulus object performed by Mr. A. C. Scott in this laboratory. He had two problems for comparison. In one the rats set up an association between the appearance of a light in one of two boxes and being fed at a point immediately over the light, i. e., a problem in which the animal learned to eat the light stimulus, we might say. This is comparable in every way with our problem in which the rats "ate the auditory stimulus" in so far as they understood the problem. The association in Mr. Scott's experiment was learned in 30 trials, the best of our rats learned in from 40 to 64 trials. In Mr. Scott's second problem, the rats were trained in the same Yerkes discrimination box to respond to the presence or absence of light by running in the proper direction through the box. The food was placed in the rear of the home box from which they started. This association was learned in not less than 120 trials. It seems reasonable to conclude, therefore, that one large factor making for rapid learning in our tests was the use of the food object for the stimulus. There are two other possibilities which would need to be seriously considered in an exhaustive study of this matter. The localization of sound may be largely an instinctive capacity; or it may be a capacity which was considerably developed in the ordinary life of the animal before the present tests were begun.

The learning of the association between source of sound and getting food fell into four main stages. In the first few trials, the rats apparently disregarded the auditory stimulus while seeking for some other cue as a guide. Visual (?) cues were used predominately, i. e., the appearing of the hand over the side of the box with food (this occurred only after the rat had made the reaction of standing up at some point), and movements of the screen. Olfactory cues seemed to be sought, for the rat frequently sniffed in all directions before responding. In the second stage, an awareness of the tapping was evidenced. This was indicated by a quick onward start when the stimulus was given. However, the animal did not seem to realize that this was a cue to the correct direction of the food, merely that it was a signal for response. The third stage of the learning was one in which hesitancy was manifested when the stimulus came. The rat would turn his head in all direct ons before deciding in which direction to go. During the rest of the reactions (embracing the fourth division of the learning of the association) the attention was placed predominately upon the proper stimulus. The progress during this stage was an increase in accuracy.

B. *Control period*—Now that the first question of the problem has been answered, i. e., can the white rat localize noise, the other two questions arise: (1) Is this an auditory reaction? (2) What factors determine accuracy of response? Twelve different control tests were put in to investigate these points.

a. *Visual controls*—To eliminate visual cues which might have been gotten from the operator, a screen of black cloth was fastened around the table, as has been indicated. Furthermore, the operator constantly stood so that the angle from the section tapped varied from trial to trial. A conclusive proof that the rat was not using the operator as a guide occurred elsewhere, e. g., in control III. when the sound of a vibrating tuning fork was used as a stimulus. Here the rat broke down entirely. If the rat had been reacting to a visual cue or even to any type of cue from the experimenter, the response would have been as accurate as before.

b. *Olfactory control* (I)—As the rats frequently stood up and sniffed in all directions before responding, odor was considered a possible cue in that food was always held in the experimenter's hand. Accordingly, small pieces of bread soaked with milk (the food used as the reward) were laid along the edges of the box in order to distribute the odor uniformly. The reaction remained normal. The odor appeared possibly to stimulate the rats in quickness of response, but no confusion arose. To serve as a further check, throughout the experiment the food was held at different angles to the point of stimulation. Final proofs were: (1) that the rat would respond accurately when the experimenter had no food in his hand; and (2) that the reaction broke down when the auditory stimulus suffered certain changes as recorded below.

c. *Kinaesthetic-tactual control* (II)—Another possible guiding cue was the kinaesthetic-tactual sensation gotten from the vibrations in the floor of the apparatus box when the stimulus was given. This had been eliminated as far as possible by fastening the stimulus board to the table and not to the main apparatus box. In control II, a layer of cotton batting 1 in. thick was placed under the apparatus box in order further to eliminate vibrations. The resulting responses were at normal accuracy. A final argument to prove conclusively the non-essential character of the vibratory cue was found in control III where the substitute stimulus, a resonator box, was held in the hand outside the screen. Extraneous vibrations were here eliminated, yet the reactions were made at a normal accuracy.

The elimination of olfactory, kinaesthetic-tactual and visual cues indicated that the response of the rat was determined by auditory factors. The problem now concerned the auditory factors determining the accuracy of localization. Other auditory timbers and pitches, the interrupted character of the stimulus and intensity were investigated.

d. *Auditory controls*—For use in later comparison, a preliminary control (III) was put in. A stimulus noise was found which was of a predominant pitch 256 d. v., and of an interrupted character. This was the tapping upon a resonator box with the pencil rubber as described above in the section on apparatus. The pitch was determined by Helmholtz resonators. The intensity has also been described previously, and was approximately equal to that of the chisel which gave the normal stimulus. Thus in this control, we have a predominant pitch 256 d. v., the interrupted character of the sound and the standard intensity. The reactions, which showed no break from the normal accuracy, were as follows:

TABLE III

Rat	6	20	4	21	22	23
Normal	6/8	2 1/8	1 1/2	1 5/8	3 3/8	1 2/8
Con. III	3	1 5/8	2 1/4	1 1/8	2 1/8	1 3/8
Con. III	1 7/8	1 3/8	2 1/8	1 1/4	2 1/2	

The three series of averages shown, were secured on three successive days.

Whether or not the pitch element *per se* of the standard stimulus was the fundamental factor in determining the localization was the next problem (control IV.) The stimulus was given by striking a tuning fork of 256 d. v., attached to its resonance box. The open end of the resonator was held at the given section of the apparatus box. Accuracy averages for this stimulus are given in table IV. The negative results indicate that the mere presence of a given pitch was not guiding the reactions. But more than this, it would seem either that the animals are unable to localize pure tone or that they are deaf (absolutely or relatively) to the one here employed under the present experimental conditions. In the light of Prof. Hunter's tests, the latter alternative is undoubtedly the correct one.[8] It is important to note here that a response of some accuracy, i. e., standing up at some point along the side of the box, was made by the rats throughout the experimentation regardless of the nature of the stimulus. Thus our method is not crucial on cases of mere sensitivity because results are stated solely in terms of accuracy. Inasmuch as the rats had been taught that food was over the side of the box, they could not be expected to remain inactive for any considerable interval of time; and, in fact, they went from one section of the box to another, standing up each time. A trial was not considered complete until the rat had stood up in such a manner.

TABLE IV.

Rat	6	20	4	21	22	23
Normal	6/8	1 5/8	1 6/8	6/8	1 5/8	1
Con. IV	4 5/8	5 3/8	8 3/8	4 1/2	6 3/8	11 1/2
Con. IV	8 5/8	7 3/7	7 6/7	8 1/4	8 1/8	6 1/8

The most significant data are derived from an observation of the animals' general behavior during the stimulation. Quickness of response, alertness, head-turnings for localization are all present when the standard stimulus are given. No attention was paid to the stimulus in control IV. The animals wandered indifferently about the box exactly as they always did between the regular tests. Several times the animals jumped when the fork was struck; however, there were no indications of an effort to localize during the continuance of the remainder of the stimulus. The same response was made when the fork was dampened and the thud of striking only was given.

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[8] Hunter, Walter S. The Auditory Sensitivity of the White Rat. *Jour. Animal Behav.*, vol. 4, No. 3, 1914.

It is worth while noting that the pitch in control IV was the same as the predominant pitch in control III. The intensities also were as close to equality as possible. Results[9] secured on the rat's ability to discriminate intensities of tone would suggest a very poor sensitivity to intensity differences. As a result of these relations, a further question arises in regard to the physiological basis for the perception of noise and the perception of tone.[10] The fact that a tone 256 d. v. is ignored while a noise 256 d. v. is reacted to accurately may indicate a separate basis for the two perceptions (or, as stated above, in the light of other work it may be due to deafness to tones of a certain pitch). The problem must be carried much further, of course, before definite conclusions can be drawn.

(Con. V) In order to test the interrupted character of the stimulus as a predominant factor in governing accuracy of response, 256 d. v., on an organ pipe was tooted. Although this too was of the same pitch as the tapping stimulus in control III and was a klang and not a simple tone, the accuracy of response was just as disturbed as with control IV. The responses seem to have been made by chance, for there was much aimless wandering and apparently no attention was paid to the sound in the great majority of cases. The reasons for the breakdown of the reactions may be either (1) inability to hear the tone 256 d. v. no matter whether simple or complex or (2) the fact that the stimulus was too different from the standard stimulus to be recognized. It must be noted, however, that the interrupted character of this stimulus elicited no better response than the continuous tone of the tuning fork while the tapping on the resonator box gave normal results. This is again in strict harmony with the results obtained by Dr. Hunter in his work (p. 221) on the auditory sensitivity of the white rat above referred to.

(Con. VI) This control, further questioned the interrupted character of the stimulus as the primary factor. The stimulus used was hissing through the teeth, a continuous sound. The matter was not investigated with sufficient thoroughness to obtain conclusive results because of lack of time. Four rats evidently heard the sound and endeavored to locate it, although their attempts were attended with very poor accuracy. The sound apparently had no meaning for the other two animals. The reactions of the first four, fall, in accuracy, between those of control III and those of control V. It seems evident that the quality of the stimulus aids in the present localizing response. Whether the rats can actually localize an interrupted sound more accurately than a continuous one, I am not prepared to say. The issue in the present case may have been one of familiarity vs. unfamiliarity.

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[9] Hunter, the last article cited, pp. 219-20.

[10] Nagel's *Handbuch der Physiol. der Menschen*, Bd. 3, S. 585, 1905.

Our attention was next directed to the place of intensity among the essential factors, determining the standard accuracy of response.

Very loud tapping with the chisel was used to test the effect of strong absolute intensity (con. VII.) The animals were startled and nervous, but their reactions were up to the normal accuracy. A light tapping (con. VIII), was next tried of an intensity not much more than just perceptible to the experimenter. Here the rats moved more slowly than usual, but maintained an attitude of alert attention. They seemed confused and in doubt, though ready to respond. The stimulus may have been below the threshold in some cases with each rat, if the matter is to be judged by aimless wandering during the giving of the stimulus. The accuracy of response was slightly decreased.

As absolute intensity, within the range used was not the essential factor, relative intensity was tested. What we were striving towards was data which would indicate that the rat's responses were governed by the relative intensity of the sound to its two ears. In the first control (IX) a double stimulus was used, necessitating two operators (Hunter and Barber). Hunter tapped heavily at the allotted section while Barber tapped lightly directly opposite. The tapping was always begun when the rat was half way between Hunter and Barber. The stimuli were given as nearly at the same moment as possible, but the louder one was always slightly earlier in that it was used for a signal for Barber to begin the lighter tapping. Thus, the difference was measured by the length of Barber's reaction time. Results, reckoned in accuracy in respect to the sections tapped by Hunter, are as follows:

TABLE V.

Rats	20	21	22	23
Normal	2 3/8	2	2 7/8	1 1/2
Con. IX	2 7/10	3	2 11/12	2 9/11
Con. IX	7/8	2 10/11	6/8	2 6/8
Con. IX	1 1/2	3 6/8	3 1/2	

As a rule the rat went to the heavier tapping, as may be seen from the results.

The question at once arose whether the first tap did not direct the rat's localization. Accordingly, a similar control was installed (X) in which the lighter tapping was begun first, acting as a guiding signal to Hunter. The control was not entirely comparable to control IX, however, for in order to insure the fact that the lesser intensity was perceived, the rat was always allowed to make a decided start towards the lighter tapping before the heavier was begun. In spite of this decided advantage for the lesser intensity, the reaction broke down in accuracy, i. e., they failed to choose the lesser intensity. This shows that the mere giving of the sound first by Hunter in control IX did not determine the response to the heavier tapping. Results reckoned in accuracy in respect to the sections tapped by Barber, are given in Table VI.

TABLE VI.

Rats	20	21	22	23
Normal	2 5/8	1 3/8	1	7/8
Con. X	8 1/8	7	8 7/8	10 7/8
Con. X	11 2/8	10	10 1/8	8 5/8

The following reaction is typical of the responses made: $\overrightarrow{B'}$ on A' $\overset{hh}{A}$ $\underline{\overset{t}{4}}$ B C D $\underline{13}$ (H 29.) Interpreted, this means that the rat was in triangle B′ (see fig. 1) headed towards the lesser intensity when the tapping was begun $\overrightarrow{(B')}$, went on to

A and then hesitated when the heavier tapping was begun hh (A), stood up at point of hesitation (4), turned towards the heavier intensity, and responded accurately to this (D13). Barber was at H29. The error, which was noted in respect to the first standing up and not in terms of the final decision was 7. From these results it is apparent that the first taps do not determine essentially the final reaction. The table further makes it clear that the rats went to the loud sound although headed toward the faint one. This is important when viewed in the light of the curve in fig. 3 which shows that the rats tended to go in the direction they were pointing.

A test further questioning the effect of the first few taps was given in control XI in which alternate tapping was made the stimulus. Hunter tapped once heavily, and then Barber tapped once less heavily, etc. The results show that as a whole the rats chose the stimuli indifferently. However, in two cases (No. 20 and No. 23) reactions to the heavier tapping predominated. No evidence was obtained that the first tap determined the point finally localized. A conclusive test of the influence of the first taps was given by control XII. In this the operator tapped only three times and then stopped, not continuing until the rat stood up as in the standard tests. Little of interest is shown in the results as tabled; however, the experiment was of importance for the observations made of the rats' reactions. The rats started as usual for the right point, but then showed great hesitancy and confusion when the stimulus stopped. Usually, however, the impetus gotten at first was great enough to secure a fair accuracy of response. The indications plainly were that the determination of the general direction of the stimulus was a matter of the first taps, while the accuracy of response was dependent upon the continued tapping. Evidently the rat utilized the whole stimulus in making his normal reaction.

Involved necessarily in this question of relative intensity is the problem of the binaural ratio. In control IX, double stimulus of two intensities, the rat was sideways to each of the operators at the beginning of the stimulus, but after the first movement, conditions were changed. Thus, no definite statement can be made in respect to the binaural factor as a cue throughout the control. A very careful effort was made to determine the relative accuracy of the rat during the standard reactions in regard to the three bodily positions, namely, when headed towards the operator, when headed away and when sideways. Many tables and curves were constructed, but no conclusions bearing upon the ratio in question were reached. (It is well in this connection to bear in mind the writer's comments upon general method made above, pp. 299.) Figure 3 brings out the not unexpected fact that the rat's greatest accuracy came when it was oriented toward the source of stimulation. When the animal was oriented sideways to the stimulus, it reacted with the next degree of perfection. The least accurate responses occurred when the animal was headed away from the stimulus.

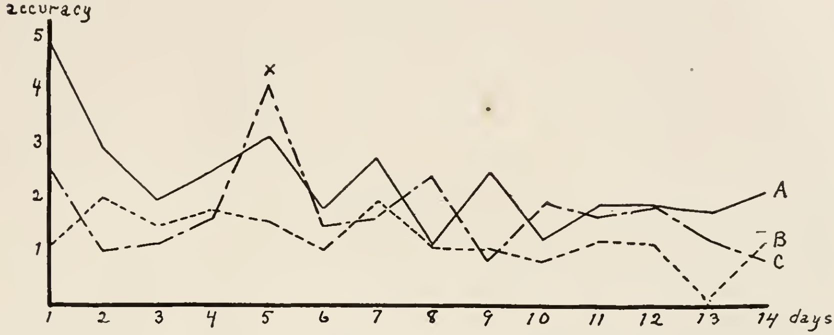

FIGURE 3. Average accuracies per day of rats 21, 22 and 23 showing, a, accuracy with head pointed away from section tapped, b, accuracy with head pointed toward section tapped, and, c, accuracy when rat was sideways to the point of stimulation. A storm occurred at x.

C. *Tests on Retention*—In order to discover the degree of permanency in the localization-association, memory tests were made upon the six rats after various intervals in which there was no training. Rats No. 4 and 6 were tested first after an interval of 40 days and then after an interval of 38 days. In each case there was essentially perfect retention. (See table VII.)

TABLE VII.

Rats	4	6
Dec. 18	2 1/2	1 1/2
Jan. 29	4 4/8	1 1/8
Jan. 30	3 6/8	3/8
Mar. 8	2 1/2	2

Rat No. 4 raised his averages, January 29 and 30, by making three bad reactions each day. The other trials however, were of normal accuracy. Rats No. 20, 21, 22, 23, were tested after a rest period of nearly a month (Feb. 11-Mar. 8) Table VIII.

TABLE VIII.

Rats	20	21	22	23
Feb. 8	3 5/8	1 3/4	1	1
Mar. 8	2 1/4	3 7/8	3 1/8	1 1/2

The younger rats, No. 20, 21, 22, 23, seemed less certain of the correct response than did the older rats, particularly No. 6, but retention was evidently present. No. 22 broke down on the first two trials given, making an average of only 5-6 on the last six. No. 21 seemed indifferent to the stimulus as a rule, although he made several perfect reactions. The small number of rats used however will not permit correlation between age and accuracy of memory. It must be remembered that although the rats were not tested during the period of rest, they still had some practice in that they would run to the sides of the cage when anyone entered the room. This however will not account for the accuracy manifested in the memory test.

SUMMARY AND CONCLUSIONS

1. The white rat is able to localize a noise with an average accuracy of from 2 to 4 inches under the conditions of the present experiment. This means, of course, 2-4 inches on either side of the point of stimulation, so that while in a single trial the accuracy is as just stated, and, let us say, to the right of the source, the next time it may be an equal distance to the left. The total space covered is, therefore, from 4 to 8 inches.

2. The association between such an accuracy of localization and food is established in from 40 to 136 trials.

3. The response is to an auditory cue, for those from vision, odor and kinaesthetic-tactual sources were eliminated from the experiment without change in the accuracy of response.

4. The auditory factor which in general determines the accuracy of localization is probably the relative intensity of the

sound to the two ears. Further tests must be made before this is established beyond question.

5. In control tests, the rats were not only unable to localize pure tones from tuning forks, but they absolutely ignored them. The same behavior was manifested toward klangs as sounded on an organ pipe.

6. A noise of 256 d. v. predominant pitch was localized while a tone of the same pitch was ignored. This has a bearing upon the problem of sensitivity to noise with insensitivity to tone, and with Prof. Hunter's work, it may point to separate bases for the perception of noise and tone.

7. There is evidence to show that if a rat is trained on an interrupted noise, it is disturbed in its accuracy of response by the substitution of a continuous noise. It is impossible to say at present whether the difference is intrinsic or not.

8. The present localizing-association was retained practically unimpaired for 40 days during which there was no training.

THE AUDITORY SENSITIVITY OF THE WHITE RAT

WALTER S. HUNTER
The University of Texas

INTRODUCTION

The present paper is a continuation of the research upon the auditory sensitivity of the white rat reported in volume 4 of this Journal[1]. That paper and the present one should be read in conjunction with an article on the localization of sound in the rat by Miss Barber[2]. The three together present a large array of data upon the sensitivity of the white rat to tones in the lower part of the pitch scale. The results have been accumulating since January, 1913, and so far as I can detect are all consistently opposed to the conclusion that the rats are sensitive to tones of the pitch used. Perhaps I should add "under the present experimental conditions," but I can see no reason for doubting that the method employed offered a perfectly fair test of the rat's ability.

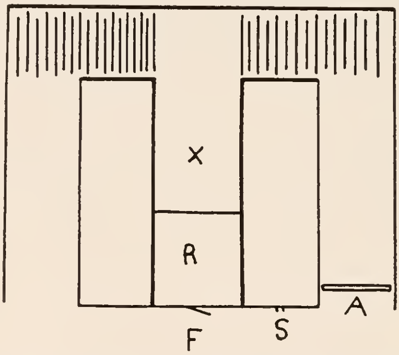

FIGURE 1. (Reprinted from volume 4, page 215, of this Journal.) T shaped discrimination box. F, food; R, release box; X, tuning fork was held above this point; A, alley stop, can be placed in either alley; S, switches.

The same apparatus and method were used in the present tests that are described in the earlier account. For the sake of clearness, it may be well to describe these again. Figure 1 is the T shaped discrimination box. The rat was expected to associate a turning to the left in order to secure food placed at F with one stimulus, and the opposite turning with another stimulus. The forks and whistles of the present tests were mounted above x. Other stimuli were given just back of the discrimination box where the experimenter stood. In the present work, the release box was not used. The animal was placed in the door at F after the stimulus had been started. Punishment and reward were used with all the rats. Unless otherwise stated in this paper, the following series of presentations (10 trials daily) were used:

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[1] Hunter, Walter S. The Auditory Sensitivity of the White Rat. *Journal Animal Behavior,* vol. 5, no. 4, 1915.

[2] Barber, Alda Grace. Localization of Sound in the White Rat. *Journal Animal Behavior,* vol. 5,.no. 4, 1915.

lrllrrrllr
rlllrrlrrl
llrrllrrlr
rllrrrlllr

The present tests were carried out during the quietest part of the day under practically ideal conditions so far as extraneous noises were concerned.

EXPERIMENTAL RESULTS

I

Four young untrained rats were tested with the tuning fork 896 d.v. In order to respond correctly in the present test, the rat should turn through the right pathway when the fork was sounded and through the left pathway when the fork was not sounded. 700 trials (70 days) were given; but the rats not only failed to learn the association, they never improved essentially during the tests. Table 1 gives the number of correct reactions out of each succeeding fifty of the 700 trials.

TABLE 1

Trials	Rats			
	29	30	31	32
50	16	25	23	27
100	22	26	27	20
150	24	23	24	25
200	22	22	17	26
250	27	22	21	23
300	24	22	20	23
350	29	25	27	18
400	26	27	28	24
450	26	30	27	27
500	21	28	18	25
550	28	25	30	31
600	25	27	28	26
650	23	24	27	28
700	28	27	22	27

II

Learning Tuning Fork Chord—Tests were made upon 4 rats in an attempt to set up an association between turning to the right and a chord composed of the tones 512 d.v. and 640 d.v. (both sounded on tuning forks) and between turning to the left and the absence of the chord. Two of the rats (Nos. 37 and 38) were untrained. The other two (Nos. 31 and 32) had gone through the tests with the fork 896 d.v. cited above. Table 2 summarizes the results. It will be seen from that that none of the rats learned the discrimination within the 650 trials given. During this time there was a slight improvement in the reactions of numbers 31 and 38; but not in the case of the other rats. No. 31 ran as high as 76% and No. 38 as high as 66% during a period of 50 trials. Although these rats had begun the tests with an accuracy of 54% and 60% respectively, it was deemed advisable to put in controls and attempt to determine the factors guiding the responses.

TABLE 2

Trials	Rats			
	31	32	37	38
50	27	30	22	21
100	27	29	26	26
150	26	26	25	23
200	26	26	23	29
250	23	26	23	22
300	22	25	29	25
350	33	31	26	24
400	28	30	21	26
450	35	23	16	19
500	38	31	26	26
550	33	24	24	32
600	31	24	26	27
650	31	31	24	33

Controls—The following controls were used: 1, chord *not* sounded. Other conditions as usual. Reaction counted wrong if it did not fit the series of presentations. 2, end-stops were placed in each alley as opposed to one alley. The chord was not sounded. Other conditions as in control 1. Using end-stops in each alley served to equalize atmospheric conditions in the two pathways. It appeared to the writer within the realm of possibility that the rats might be able to detect a freshness of the air through the open pathway which would not be present in the closed one. Their behavior was hesitant and harmonized a priori with such an hypothesis. As soon as a rat had chosen the proper pathway, the end-stop on that side was quickly and noiselessly removed so that by the time the rat reached the alley on the side of the box a free exit was offered him. 3, no end-stops; no punishment; no chord sounded. Reactions right when they fit the series of presentations. If the rat chose wrongly, he was confronted by an open pathway to the food. In this control, even the punishment due to a blocked pathway was removed. This and the following control were used to test the rôle of kinaesthetic factors or position habits. Control 3 sought the character of these habits when uninfluenced by punishment. 4, conditions the same as in control 3, save that the electric shock was used when errors occurred A quick interposition of the end-stop prevented the animal from reaching food when it chose the wrong alley. It was thought that the introduction of such punishments might lead to changes in the position habit as revealed in control 3. 5, no end-stops; everything else normal (as in standard learning series.)

The results attending the introduction of the controls are

TABLE 3

Nature of test	Number of trials	Rats		
		31	32	38
Con. 1	20	65%	65%	65%
Normal	20	70%	65%	60%
Con. 1	20	75%	65%	65%
Con. 2	20	50%	60%	....
Con. 3	30	....	....	53%
Normal	20	70%	60%	....
Con. 1	20	....	....	60%
Con. 2	20	45%	....	....
Con. 2	30	....	....	46%
Con. 3	20	....	65%	....
Normal	40	72%	....	....
Con. 1	10	....	....	40%
Con. 4	20	....	40%	....
Con. 1	20	....	60%	....
Con. 2	40	52%	....	....
Normal	50	68%	....	....
Con. 1	30	63%	....	....
Normal	50	74%	....	....
Con. 5	10	80%	....	....
Con. 3	20	65%	....	....
Con. 1	30	66%	....	....
Normal	60	70%	....	....
Con. 2	20	60%	....	....

Records are given in their chronological order. Correct responses are given in per cents. Rat 37 was not tested.

given in table 3. The significant fact from the standpoint of tone sensitivity is that the animals maintained their relatively high percentage (60% to 75%) of correct reactions *when the chord was not sounded.* This is conclusive proof that the slight improvement in accuracy found in the learning records in no way depended upon tone sensitivity. In other words the rats showed themselves as unable to respond to a fairly complex klang as to a simple tone.

The remaining controls were not worked out in detail with rats 32 and 38. The diary records indicate clearly that the responses were governed by position habits. Rat 31 was tested more fully. There are two chief points of interest in the data secured through these controls, (1) a high percentage of correct responses can be made on the basis of position (kinaesthetic) habits even though the series of presentations is very complex and cannot be said to be learned; and, (2) simple alternation seems to be the fundamental position habit. Although data with control 4 are lamentably lacking, it seems probable, from reasons given below, that the position habits were affected by punishment.

Control 2 did not disturb the reactions of rat 32, but did slightly those of rat 38. Rat 31 was undoubtedly affected by closing up both alleys. This was not a disturbance due to a changed visual (brightness) condition in the alleys, because the relation of the apparatus to the source of light precluded this. It would seem that normally rat 31, and probably rat 38, was greatly dependent upon the atmospheric conditions (freshness and better air circulation) of the two alleys. This is only advanced as a probability. Tests made directly upon the rat's ability to discriminate such stimuli offer the only definite approach to a solution of the question.

Controls 4 and 5 had no effect upon the animals tested. The poor percentage made by rat 32 with control 4 is to be accounted for entirely in terms of peculiar position habits. Part of the time a simple alternation was present; part of the time there was alternation after a success only. Control 3, by leaving both alleys of the apparatus open, permitted the rats' position habits (or kinaesthetic controls) to assert themselves in an unmodified way. In every case where this control was used, the rats tended to fall back immediately upon the method of simple alternation between the two alleys. For example the series llrrllrrlr is given and the animal chooses as follows lrlrlrrlr. The reaction is 70% correct and yet there is only one reversal (underscored) of the series of alternations. Again the series rllrrrlllr is given and the rat alternates with no reversal making 60% correct. Another rat given this latter series alternated regularly save that on the 3rd, 4th and 5th trials he went to the right hand box. In the regular series (on learning), there were more reversals and there was also a marked tendency to reverse after each success only. Inasmuch as this last type of behavior was not present when punishment and the end-stops were not used, it seems probable that these factors produced the behavior by modifying the simple alternation position habit.

The moral of these controls lies in pointing out the necessity of being on one's guard against complex position habits which otherwise might be taken as evidence of discriminative ability.

Many tests in comparative psychology which have been intended primarily as tests of discrimination have been vitiated because they have required the animal to *localize* the stimulus in order to give evidence of sensitivity. Hence when negative results are secured it is not known whether the animal was insensitive or whether it was simply unable to localize the stimulus. The method adopted in the present tests does not involve a localization factor (Johnson's work has this merit also.)[3] Hence the negative results secured when working with the pure tones 256 d.v. and 896 d.v. and with the chord 512 d.v. plus 640 d.v. are of great significance.

III

Learning Whistle—Tests were now begun on four untrained rats (Nos. 44, 45, 46 and 47) using as a stimulus 3906.17 d.v. on a Galton whistle. The whistle was held in clamps above the experiment box where the forks had been and was turned in such a manner that air currents were not directed downward upon the animals. It was sounded by blowing (with the experimenter's mouth) through a long tube. The standard intensity of this tone, when measured with a water manometer, was secured with a pressure of 16 cm. We may call this whistle,

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[3] Johnson, H. M. Audition and Habit Formation in the Dog. *Behav. Mon.*, vol. 2, no. 3 1913.

whistle A. Later during the control tests another Galton whistle was used, whistle B. B was a new instrument and was accepted as standard. Its tonal divisions were lower in pitch than those of A, so that 3413.3 on A was equaled by 3906.17 on B, using the same air pressure. (These measurements are very close approximations.) Pitches are always stated in terms of B.

In these tests the problem set the rat was the associating of a turn to the right for food with the whistle tone and a turn to the left for food with the absence of the whistle. 10 trials daily were given with punishment and reward. The period of learning plus the control period extended from April 17, 1914 to October 17, 1914.

Table 4 gives the results on learning. Rat 44 was dropped at the end of 650 trials because of an incurable habit of always going to the right whether the stimulus was sounded or not.

TABLE 4

Number of Correct Reactions in Each Succeeding 50

Trials	Rats			
	44	45	46	47
50	23	21	19	19
100	18	26	21	25
150	28	24	13	22
200	28	32	19	25
250	29	25	14	21
300	28	21	32	28
350	28	26	27	23
400	28	24	33	24
450	29	32	35	23
500	27	27	26	29
550	27	29	28	22
600	32	32	33	39
650	10	35	34	29
700	..	35	40	37
750	..	43	28 of 30 trs.	37
800	..	39	..	42
850	..	41	..	39
900	..	36	..	39
950	..	46	..	43
960	..	10	..	9

The problem was considered learned by rat 46 at the end of 730 trials, and by rats 45 and 47 at the end of 960 trials. Controls were then instituted.

Controls—The following is a summary statement of the eleven controls used in analyzing the animals' reactions:

1. Leave off end-stops. Everything else normal.
2. Do not sound whistle. Everything else normal. Reactions correct if they fit the series.
3. Whistle is blown so as to give "rush of air sound" but no tone. The "rush of air sound" is probably twice as loud as it is when it accompanies the whistle tone. All else is normal.
4. Make "rush of air sound" with lips. Intensity equal to that of control 3. Care taken that air currents do not reach rats.
5. Clap hands in place of giving whistle tone. Medium intensity.
6. Two holes are bored in wall of room. A rubber tube passes through one from the experimenter to a Galton whistle placed in the adjoining room. The mouth of the whistle is set close in front of the second hole with a paper reflector directing the sound back into the experiment room. The whistle is set for the tone 3906.17 d.v. and is sounded in place of the whistle over the apparatus box. Everything else is normal.
7. Whistle over the apparatus box is used at the same intensity as the tone of control 6. The two were matched by sounding first one and then the other. The pressure in the water manometer was 4 cm.
8. Fork 1280 d.v. sounded in place of whistle and at a little more than the intensity of control 7.
9. The whistle over the apparatus box set at 1280 d.v. and sounded under normal conditions.
10. Fork 1152 d.v. and fork 1280 sounded as a chord in place of normal whistle tone. Slightly greater intensity than control 8.
11. The whistle used in control 6 was substituted for the whistle usually sounded over the box. The normal intensity and pitch were given.

Inasmuch as I regard the full presentation of the records on these controls as a matter of great importance, tables 5, 6 and 7 are given in the appendix. In these the reader will find a chronological statement of controls and results for each of the three rats. Here in the body of the paper, I shall gather together all of the tests made upon a given control irrespective of the relative times at which the tests were made.

Control 1.—When the alley-stops were not used the animals reacted normally.

Control 2.—All of the rats failed in their reactions when the whistle was not sounded. This demonstrates clearly that there was some cue involved in the presentation of the whistle stimulus that determined the reactions. A reference to table 6 (appendix) will reveal the fact that I have not included in No. 46's record 30 trials secured with this control during which 70% of the reactions were correct. This rat's dependence upon extra-auditory cues was very temporary. This is indicated by the fact that only 50% and 60% were made with control 6 just before and by the further fact that just succeeding the 70% with control 2 the rat fell back to 55% with the same control. I do not know what cue was used during that brief period.

Rat 45	20 trials	60% correct
Rat 46	40 trials	52% correct
Rat 47	20 trials	65% correct

Controls 3, 4 and 5.—All of the rats succeeded when the noise of rushing air was substituted for the whistle tone. One cannot argue from this that the rats did not hear the tone, although this is a possibility—in fact a probability when considered in connection with the other facts here brought together. Judging from this control alone or in connection with controls 4 and 5, an alternative hypothesis is evident, viz., the rats reacted to any auditory stimulus which stood out clearly at the moment of response. An inspection of table 7 (appendix) will indicate that rat 47 was disturbed slightly at the beginning of each series of tests with control 3. Each time, however, the disturbance quickly passed away. There was less disturbance with control 4. When control 5 was used, there was a complete breakdown at first; but later on in an isolated test period of 20 trials, 85% of correct reactions were made.

Control 3	Rat 45	40 trials	82% correct
	Rat 46	40 trials	82% correct
	Rat 47	70 trials	77% correct
Control 4	Rat 45	40 trials	80% correct
	Rat 46	30 trials	86% correct
	Rat 47	40 trials	77% correct
Control 5	Rat 45	20 trials	80% correct
	Rat 46	50 trials	80% correct
	Rat 47	50 trials	66% correct

Rat 46 (table 6, appendix) was only disturbed with the 5th control, and this was speedily overcome—raised from 60% to 85%. Rat 45 was not disturbed by either of the three controls.

The net result of these controls is that the rats are able to respond correctly to two very different noises when these are given in the place of the standard whistle. So far, then, it is certain that although the rats were dependent in their reactions upon the auditory stimulus, this was certainly not of a specific nature. This is in harmony with the data set forth in the two companion papers (Hunter and Barber, above cited). It was necessary, therefore, to work further in order to show that the tonal element in the whistle was or was not effective.

Controls 6 and 7.—The only crucial test on the tonal element that could be made with the whistle depended upon ruling out any accompanying noise. There was only one method that was at all feasible. That was to remove the whistle to such a distance that distance itself would eliminate the extraneous factors. Such a test can only be suggestive and never conclusive: (1) It is impossible to tell whether or not the noise has been eliminated for the rat. (2) Distance not only cuts out the noise, but also cuts out overtones and lowers the general intensity of the stimulus. The first is the weighty objection. The second I believe has little or no weight because: (a) from the work on chords cited above and to be cited below (control 10), it is doubtful whether tonal complexity means much for the rat's reactions; and (b) control 7 indicates that the lowering of general intensity is non-effective. This point is made certain for pure tones, if not for klangs, by (work cited on pp. 219-220 of) the author's previous paper on the auditory sensitivity of the rat.

Control 6	Rat 45	50 trials	56%
Control 7	Rat 45	70 trials	78%
Control 6	Rat 46	90 trials	67%
Control 7	Rat 46	80 trials	81%
Control 6	Rat 47	50 trials	60%
Control 7	Rat 47	70 trials	71%

The numerical data just given indicate clearly that the rats broke down for control 6. We have just pointed out the possible reasons for such behavior,—the most probable one being the elimination of noise by distance. We must now indicate why rats 45 and 46 were disturbed by control 7 where the normal whistle stimulus was simply sounded at a less intensity than usual. When the standard whistle is decreased in intensity the change is much the same as occurs when the whistle is taken to a distance, i. e., the noise accompanying the whistle is decreased in intensity if not eliminated and similar changes most probably occur among the overtones. Therefore, even from an a priori standpoint, one need not be surprised that the reactions with control 7 were of less than normal accuracy. The important fact is that they were significantly better than the reactions with control 6. Inasmuch as the stimuli in the two controls, were, for the experimenter, extremely similar in respect to intensity magnitude, it seems most probable that the rats were governed in their responses by the noise usually accompanying the whistle tone. There is no evidence that the animals heard the stimulus in control 6.

Controls 8 and 9.—In a previous experiment rats that had been trained to respond to the noise of handclapping were unable to make discriminative responses when tones were substituted for the standard stimulus, although the substitution of other noises was attended by positive results. The same facts appear here with the piston whistle tests. It will be seen from the numerical summary here given that all of the rats failed unqualifiedly to respond in control 8 to the tuning fork 1280 d.v. Rat 46's percentage for control 8 would be only 54, if 20 trials were ruled out when the animal was responding to extra-auditory cues. (See above page 290, and appendix table 6.)

Control 8	Rat 45	70 trials	60% correct
Control 9	Rat 45	20 trials	90% correct
Control 8	Rat 46	70 trials	61% correct
Control 9	Rat 46	20 trials	85% correct
Control 8	Rat 47	80 trials	60% correct
Control 9	Rat 47	20 trials	85% correct

There was no tuning fork available whose vibration rate was at or above 2000 d.v. It was therefore impossible to use a fork of a pitch equal to the standard whistle. However, the next best thing was done. The whistle pitch was lowered in control 9 to the pitch of the fork used in control 8 (1280 d.v.). The data given above indicate that the rats reacted as well to this whistle pitch as to the standard in spite of the fact that only failure attended the use of the tuning fork.

Control 10.—The difference in the results obtained in controls 8 and 9 may have been due to differences in the complexities of the stimuli. This is true although long tests were made (as described above) in a fruitless endeavour to set up a discriminative reaction to a common chord, 512 d.v. plus 640 d.v. In the present control the chord 1152 d.v. plus 1280 d.v. was sounded on forks in place of using the standard whistle. The animals again failed in their reactions. If, in spite of the fact that the results of my experiments indicate that differences in tonal complexity are not utilized, later studies should show that my tests did not offer complex enough klangs, they will be very interesting in establishing a threshold for tonal sensitivity on the basis of tonal complexity.

Rat 45	50 trials	66%
Rat 46	40 trials	57%
Rat 47	40 trials	57%

Control 11.—This control was introduced to supplement controls 6 and 7. In order to rule out the possibility that the data there obtained were due to intrinsic differences in the whistles, the one used in control 6 was placed over the apparatus in place of the standard whistle and was sounded at the intensity of control 7. Rat 47 was not tested, but the other two made 90% and above. It may be concluded from this that for the rat no intrinsic differences in the whistles were functionally effective.

IV

In instituting and continuing the series of tests with piston whistles as just set forth, the writer was influenced by two principle motives: (1) Uniform failure had waited upon all of the work with tuning forks. It was hoped that with the whistle positive data might be secured whose analysis would throw light upon the problem of tone sensitivity. (2) It has been suggested recently, notably by Yerkes, that where a difficult discrimination is required of an animal, training should first begin with a complex easily discriminable and gradually be directed toward the aspect upon whose presence the problem centers. When the present whistle tests had been begun primarily with the first intention, it was thought advisable to extend them in the light of the second.

In the work published in volume 4 of this Journal, rats that had responded to noise by turning to the *left* were later given from 350 to 520 trials in an endeavour to force them to turn to the *right* for the tone 256 d.v. sounded on the fork. No one of the rats showed improvement during this interval.

In the present instance rats 45, 46 and 47 had been trained to react successfully to the whistle by turning to the right. They were then given a series of tests with the tuning fork 256 d.v. in an attempt *to train them to turn to the right for the fork tone also.* A new series of presentations was employed as follows:

rllrrrllrl
rrlrllrllr
lrrlrrlrll
llrlrrlrlr

Three hundred trials were given; but there was no improvement in the reactions from first to last. The following table is a summary of this fact. These results lend further indirect confirmation to the conclusion drawn above that the rats were depending upon noise in the whistle complex and not upon tone. The present results are also very striking when we compare them with the rat's ability to react to noises which are very dissimilar, from the experimenter's point of view, to those with which it has been trained. To quote from the previous paper, pages 221-222, "All of the tones given were for some reason very different from the noises. .. .. Inasmuch as the animals reacted in the same manner to all of the noises, it is certainly a striking fact that none of the tonal stimuli given were classed as noises."

TABLE 5

Trials	Rats		
	45	46	47
50	25	28	27
100	26	32	28
150	29	28	28
200	29	28	27
250	25	23	27
300	27	31	31

V

Retention Tests.—Forty-one days (for rats 46 and 47) and 45 days (for rat 45) after the tests on control 11 for rats 45 and

46 and on control 7 for rat 47, the animals were again tested on their ability to turn to the right for the whistle tone. During the interim, the work just described on the fork 256 d.v. was carried on. Whether this could have affected the retention will depend upon the rat's sensitivity to the tone in question. If he can hear the tone, there should at least be no decline in the accuracy of response due to the training on the fork. If, however, he cannot hear the tone, the training upon turning to the right for the fork will tend to break down the normal reaction of turning to the right for the whistle. The results show a decrease in accuracy of response for two rats. Rat 47 reacted normally. These results may be due either to the effect of time intervals or to habit interference.

Rat 45	Rat 46	Rat 47
Control 11, 90% of 20 trs.	Control 11, 100% of 20 trs.	Normal, 90% of 10 trs. Control 7, 80% of 10 trs.
Retention tests; per cent correct of 50 trials given		
72%	76%	80%

VI

The most significant data secured in the work upon the auditory capacity of the white rat may be summarized as follows, (data are here mentioned which were included in the author's previous paper and in Miss Barber's work.):

A. *Crucial Evidence Upon Tone Sensitivity:*

(1) The tone 256 d.v. sounded on the tuning fork was not discriminated: (a) by 6 untrained rats after 700 trials, (b) after 700 trials, by one rat that had learned to react to hand clapping within 400 trials; (c) after 350, 410 and 520 trials respectively by three rats that had been trained previously to respond to hand claps; (d) after 300 trials by 3 rats that had been trained to respond to a whistle.

(2) The tone 896 d.v. sounded on a tuning fork was not discriminated by 4 untrained rats after 700 trials.

(3) The chord 512 d.v. plus 640 d.v. sounded on forks was not discriminated; (a) by 2 untrained rats after 650 trials; and (b) after 650 trials by 2 rats that had been trained upon the

tone 896 d.v. If we count the difference between that tone and the present chord as sub-limnal, these 2 rats were given 1350 trials with no evidence of discrimination.

B. *Evidence Bearing Upon Tone Sensitivity Which While Not In Itself Crucial Is Yet Of The Greatest Significance*:

(1) Four rats trained to react to a whistle tone of 3906.17 d.v. would not react to a tuning fork chord (1152 d.v. plus 1280 d.v.), or to the fork 1280 d.v. when these were each substituted for the standard stimulus. When a whistle of the same pitch was sounded in an adjoining room so that distance probably eliminated the noise factor, the rats failed; although they made a significantly larger per cent of correct reactions when the standard stimulus was decreased in intensity to match the intensity of the distant whistle.

Further these same rats reacted properly when either of the following noises were substituted for the standard whistle; (a) the rush of air through the whistle; (b) sound of "rush of air" made with lips; and (c) clapping of hands. The rats reacted successfully to 1280 d.v. on the standard whistle *but failed when the same pitch* was sounded on a tuning fork.

(2) Three rats trained to react to hand clapping reacted successfully to the following noises when these were substituted: rattling of paper, dropping sunflower seed on tin, scratching on wood, drumming on the table with the fingers, rubbing two pieces of board together, hissing through the teeth, and rattling nails in a glass. These rats failed when the following tones were sounded in place of the hand claps: (a) 1024 d.v. on fork; (b) 256 d.v. on organ pipe sounded steadily; (c) b sounded in toots; (d) 1024 d.v. sounded steadily on organ pipe; (e) d sounded in toots; and (f) 341.3 d.v. on the organ pipe sounded steadily.

(3) Six untrained rats failed (after from 575-800 trials) to discriminate a very intense from a very faint sounding of the fork 256 d.v.

(4) Rats trained to localize a tapping noise ignored: the fork 256 d.v.; and the same pitch tooted upon an organ pipe. They responded to a noise made by tapping with the rubber end of a lead pencil upon the resonator box of the fork 256 d.v. This gave an interrupted noise of the same predominant pitch as the fork.

CONCLUSIONS

(1) There is a practical insensitivity to many pitches in the lower region of the scale for the white rat. This apparently goes along with a sensitivity to noises of the same predominant pitch.

(2) Differences in tonal complexity and intensity may be considerable without making discrimination possible.

(3) Apparent reactions to tone are most probably made to accompanying noises.

(4) If, after all, there is a sensitivity to tonal stimuli as here tested, then, for the rat, tones and noises are very different classes of stimuli.

APPENDIX

Chronological statement of controls used with rats in whistle tests.

TABLE 5

RAT 45

Nature of test	Trials	Per cent correct
Con. 1	10	90
Con. 2	20	60
Normal	20	90
Con. 3	20	80
Normal	10	100
Con. 4	20	80
Normal	60	83
Con. 6	10	60
Con. 7	20	85
Normal	10	100
Con. 6	10	60
Con. 8	10	50
Con. 7	10	80
Con. 8	10	50
Con. 6	10	50
Normal	30	83
Con. 9	10	90
Con. 8	20	70
Con. 10	50	66
Normal	20	95
Con. 8	20	60
Con. 3	20	85
Con. 4	20	80
Con. 5	20	80
Con. 6	20	55
Con. 7	40	75
Con. 11	20	90

TABLE 6

RAT 46

Nature of test	Trials	Per cent correct
Con. 1	20	90
Con. 2	10	50
Normal	40	77
Con. 2	10	50
Con. 1	10	80
Con. 3	10	70
Normal	70	63
	last 40	100
Con. 3	10	100
Con. 4	10	100
Con. 5	10	60
Normal	30	90
Con. 5	20	85
Con. 6	10	60
Con. 7	10	50
Normal	10	100
Con. 6	20	75
Con. 7	20	85
Con. 6	10	50
Con. 7	10	80
Con. 6	10	60
Con. 7	10	100
Con. 8	20	80*
Con. 2	10	100*
Con. 2	20	70*
Normal	10	80*
Con. 2	20	55*
Con. 8	10	60
Con. 8	20	50
Normal	20	90
Con. 9	20	85
Con. 10	40	57
Normal	20	95
Con. 8	20	55
Con. 3	20	80
Con. 4	20	80
Con .5	20	85
Con. 6	40	72
Con. 7	30	83
Con. 11	20	100

* For comments upon these controls see body of text.

TABLE 7

RAT 47

Nature of test	Trials	Per cent correct
Con. 1	10	80
Con. 2	20	65
Normal	20	90
Con. 3	20	60
Con. 3	20	95
Normal	10	90
Con. 4	10	60
Con. 4	20	85
Con. 6	10	60
Con. 7	10	50
Con. 6	10	60
Con. 7	10	50
Normal	20	80
Con. 7	10	60
Con. 5	10	50
Con. 5	20	55
Normal	20	85
Con. 9	10	90
Con. 8	10	60
Con. 9	10	80
Con. 8	10	60
Con. 10	20	60
Con. 8	10	50
Normal	20	80
Con. 10	20	55
Con. 8	50	62
Con. 3	10	76
Con. 3	20	85
Con. 4	10	80
Con. 5	20	85
Con. 6	30	60
Con. 7	20	100
Normal	20	95
Con. 7	10	60
Normal	10	50
Normal	10	90
Con. 7	10	80

THE RELATION OF STRENGTH OF STIMULUS TO RAPIDITY OF HABIT-FORMATION IN THE KITTEN

J. D. DODSON
Central College, Pella, Iowa

For this study of the relation of strength of stimulus to rapidity of learning I have used the method which Yerkes and I found satisfactory in our similar study of the dancing mouse.[1] But instead of requiring the kitten to choose between white and black, as in the case of the mouse, I required it to discriminate between light and dark—that is the kitten had to choose in the light-dark series instead of the white-black series. Irrespective of the relative positions of the two boxes the subject had to choose the light one. Should the kitten enter the dark box it received an electric shock, and was never allowed to escape by passing through the same.

The apparatus was very much the same as that used with the dancer (for general construction see figures 1 and 2, page 460, of the article referred to above). The experiment box was divided into a nest box, an entrance chamber and two electric boxes. The entrances and exits to the electric boxes were 9 by 9 cm. each. The electric boxes were placed in the circuit of a constant electric current. In this circuit was a double key by which the experimenter could direct the current through either box he might wish. The inductorium and resistance coil were placed in an adjoining room thus eliminating all the noise of the constant buzz of the inductorium. The current was furnished by a storage battery which was kept constant at a voltage of 19.5 and amperage of 4. To govern the amount of light entering the electric boxes, the entire end of the experiment box containing the electric boxes was covered, and two openings cut in the cover, one directly over each electric box. In order to prevent the kitten from seeing the opening in this cover as it entered the electric box, a platform 25 cm. wide was placed 12 cm. from the top of the box and directly over the wires. The experimenter determined which electric box should be light and which should be dark by placing a cover over one of the openings. The cardboard cover was shifted in the same order as in the experiment with the dancer (table 1, page 461).

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[1] *Jour. of Comp. Neurol. and Psy.*, 1908, **18**, 459-482.

The kitten was placed in the nest box by the experimenter and a plain glass cover put over the box to prevent the kitten's climbing out at the top. The only way left for the animal to escape was to pass through an opening into the entrance chamber and thence through the electric box and out at an exit at the rear of the experiment box. A mirror was placed so that the experimenter could see the kitten without the kitten's seeing the experimenter. The play instinct caused the kitten to be very restless and, thus, it soon attempted to make its escape. If it chose the light box it was allowed to pass through undisturbed; but should it choose the dark one it received an electric shock. This shock usually caused a hasty retreat, but should the animal attempt to pass on over the wires the experimenter forced it to return into the entrance chamber not allowing it to escape through the dark box.

Each of the 18 kittens used was given ten tests each day until it succeeded in choosing the light box correctly for three consecutive days. If the kitten should enter far enough into the dark box to receive a shock it was recorded as a mistake but in order for the trial to be counted a test the kitten must escape from the box. The principal motives for the kitten's escape were the instinct of play and the gregarious instinct. The animals were given their usual meals during the day, but I always fed them at the close of the experiment.

Each kitten was just six weeks old when I began to train it. All were of the same stock of cats. I conducted three sets of experiments. First set was done with the condition of visual discrimination rather difficult, using a medium and relatively strong stimuli. For the second set the condition of discrimination was less difficult. For the third set the condition of visual discrimination was fairly easy.

At the beginning of the training of a set of kittens I allowed each one to pass through the electric boxes a number of times without turning the electric current in either box. This was to teach the kitten that there was a way out of the box and also to tend to establish in the animal the habit of escaping. During this preliminary work I shifted the cardboard from side to side to determine whether or not the kitten had any preference for the light or dark box and found no preference shown.

The difference in the amount of light entering the two electric boxes was that which would pass through an opening in the cover 23.5 by 13 cm. Of this opening 16 by 13 cm. was over the platform which was 12 cm. below the top of the box. For the medium stimulus a current with a voltage of 19.5 and an amperage of 2.5 was run through an inductorium with a coil set 3.6 cm. from the closed end. For the strong stimulus a current was used with the voltage and the inductorium the same as for the medium stimulus but the amperage was 3.5.

Results of experiments of set 1. Tables 1 and 2 show detailed results of set 1. At the top of each table are given the numbers of the kittens which were used under the conditions named in the heading of the table. The first column gives the number series; the other columns give the number of errors and the average of errors made by male and female and also the general average; while the last line gives the total number of trials and their average for perfecting the habit.

TABLE 1

THE RESULTS OF EXPERIMENTS OF SET I, MEDIUM STIMULUS

(VOLT. 19.5, AMP. 2.5)

	Males			Females			
Series	No. 1	No. 5	Average	No. 2	No. 6	Average	General Average
1	6	4	5	7	4	5.5	5.25
2	2	2	2	2	3	2.5	2.25
3	4	2	3	6	4	5	4
4	4	2	3	4	3	3.5	3.25
5	1	3	2	1	2	1.5	1.75
6	0	4	2	1	2	1.5	1.75
7	1	1	1	0	2	1	1
8	1	1	1	1	2	1.5	1.25
9	0	0	0	0	3	1.5	0.75
10	0	0	0	0	0	0	0
11	0	0	0	0	0	0	0
12					0	0	0
Total No. of trials	80	80	80	80	90	85	82.5

TABLE 2

THE RESULTS OF EXPERIMENTS OF SET I, STRONG STIMULUS

(VOLT. 19.5, AMP. 3.5)

Series	Males			Females			General Average
	No. 3	No. 7	Average	No. 4	No. 8	Average	
1	6	4	5	5	4	4.5	4.75
2	5	3	4	4	4	4	4
3	2	3	2.5	4	4	4	3.25
4	4	3	3.5	4	2	3	3.25
5	7	2	4.5	2	3	2.5	3.5
6	3	2	2.5	3	4	3.5	3
7	2	2	2	2	4	3	2.5
8	1	2	1.5	1	4	2.5	2
9	0	4	2	1	3	2	2
10	2	1	1.5	0	1	0.5	1
11	0	0	0	0	3	1.5	0.75
12	0	2	1	0	2	1	1
13	0	0	0		0	0	0
14		0	0		0	0	0
15		0	0		0	0	0
Total No. of trials	100	120	110	90	120	105	107.5

Special conditions of set 11. The visual discrimination was made less difficult by putting a cover over 15 by 40 cm. of the nest box and by cutting out the openings over the electric boxes until they were 36 by 13 cm. instead of 23.5 by 13 cm. The strengths of stimuli were the same as in set 1, but only two kittens were used for each strength.

TABLE 3

THE RESULTS OF EXPERIMENTS OF SET II, MEDIUM STIMULUS

(VOLT. 19.5, AMP. 2.5)

Series	Male, No. 9	Female, No. 10	Average
1	5	5	5
2	4	5	4.5
3	4	2	3
4	3	2	2.5
5	2	1	1.5
6	1	1	1
7	0	0	0
8	0	0	0
9	0	0	0
Total number of trials	60	60	60

TABLE 4

RESULTS OF EXPERIMENTS OF SET II, STRONG STIMULUS

(VOLT. 19.5, AMP. 3.5)

Series	Male, No. 11	Female, No. 12	Average
1	4	6	5
2	4	2	3
3	3	4	3.5
4	3	2	2.5
5	3	1	2
6	2	0	1
7	0	0	0
8	0	0	0
9	0		0
Total number of tests	60	50	55

Special conditions of set III. The nest box and entrance chamber were lined with black cardboard and the electric boxes were lined with white. The openings over the electric boxes were cut out till they were 40 by 18 cm. and the nest box and entrance chamber were covered with cardboard all but an opening 20 by 40 cm. Thus the condition of visual discrimination was made fairly easy. The stimuli used were the same as in the previous experiments, but with one additional stimulus.

TABLE 5

RESULTS OF EXPERIMENTS SET III, STIMULUS WEAK (VOLT 19.5, AMP. 1.5)

Series	Male, No. 13	Female, No. 14	Average
1	5	6	5.5
2	5	4	4.5
3	4	4	4
4	3	2	2.5
5	2	3	2.5
6	3	2	2.5
7	1	1	1
8	1	0	0.5
9	0	0	0
10	0	0	0
11	0		0
Total number of trials	80	70	75

TABLE 6

Results of Experiments of Set III, Stimulus Medium

(Volt. 19.5, Amp. 2.5)

Series	Male, No. 15	Female, No. 16	Average
1	4	6	5
2	5	4	4.5
3	4	4	3.5
4	4	2	3
5	3	2	2.5
6	0	0	0
7	0	0	0
8	0	0	0
Total number of trials	50	50	50

TABLE 7

Results of Experiments of Set III, Stimulus Strong

(Volt. 19.5, Amp. 3.5)

Series	Male, No. 17	Female, No. 18	Average
1	5	4	4.5
2	4	3	3.5
3	1	1	1
4	0	1	0.5
5	0	0	0
6	0	0	0
7	0	0	0
Total number of trials	30	40	35

Possibly no one realizes more fully than the experimenter certain crudities of method in this experiment, but still there are some things of interest to the animal psychologist. And if any conclusions may be drawn from the use of so few animals those conclusions are in accord with previous findings in the dancer.

Conclusions. 1. The rapidity with which kittens acquire the habit of always choosing the light box may be seen from the following results: Under fairly difficult conditions of learning, with a medium stimulus it took on the average 82.5 trials for the kitten to perfect a correct habit, and with a strong stimulus 107.5 trials; under less difficult conditions it took 60 trials with a medium stimulus, and 55 trials with a strong stimulus; and under fairly easy conditions it took 75 trials with a weak stimulus, 50 trials with a medium stimulus, and 35 trials with a strong stimulus.

2. The relation of the painfulness of the electrical stimulus to the rapidity of habit formation depends upon the difficultness of the visual discrimination.

3. When the discrimination is difficult the medium strength of stimulus was found to be the more favorable to habit formation; but when the discrimination is less difficult the difference between the unpleasant and the very unpleasant stimuli is not marked. When the discrimination is easy the rapidity of habit formation increases as the unpleasantness of the stimuli is made greater, at least within certain limits.

NOTES

THE MATING OF LASIUS NIGER L.

C. H. TURNER
Sumner High School, St. Louis, Mo.

It was three o'clock on the afternoon of September the seventeenth, 1913. For two days we had been having frequent showers; even then, although the sun was shining brightly, there were numerous clouds in the sky, any one of which, without a moment's notice, might float before the sun. The temperature was only 78 degrees Fahrenheit; but, compared with the 73 degrees of the afternoon of the sixteenth and with the 63 degrees of the afternoon of the fifteenth, it seemed quite warm. The numerous nests of the ant *Lasius Niger L.*, which had long existed, unnoticed, beneath the pavements and in the vacant lots of St. Louis, had suddenly been rendered conspicuous by the restless myriads of gigantic virgin females, miniature males, and small workers that were swarming from them and forming agitated masses of ants about each entrance.

On viewing this periodically repeated phenomenon one is tempted to assume that the ants have suddenly become negatively geotactic and positively phototactic; and this hypothesis is strengthened by the fact that, on the evenings following such an occasion, females of the species may be captured at the street lights. It may have been a negative geotropism, it certainly was not a positive phototropism which urged the ants from the nest; for the sunlight does not penetrate into the nests. A prolonged and careful observation of the virgin females and neuters of a nest situated at the foot of a large grape-vine revealed conditions that do not harmonize with so simple an explanation. If the behavior of these ants were wholly a negative geotropism, or a positive phototropism, or a combination of both, then they should have climbed ever upward until the tips of the twigs were reached; but, that is not the way they behaved. Along the lower four feet of that vine the females and neuters were constantly ascending and descending. To watch the agitated promenading of these restless ants up and down the stem, was to be convinced that these activities of the unmated females and of the neuters were not merely a tropism. Evidently the physiological changes caused by the maturing of the sexual powers had initiated a restless meandering.

I am not certain how the behavior of the males should be interpreted. The leaves of the grapevine and the tops of other uprights were black with them. The stem of the vine supported countless numbers of them; but my attention was so completely concentrated upon the movements of the females and neuters that I did not notice whether the males were moving ever upwards or to and fro.

The concentration of attention upon the virgin females was for the purpose of observing every detail of their mating behavior. Several females were watched from the time they left the nest until they flew away. Both on the ground and on the grapevine, they roamed in and out among the males, jostling them to the right and to the left, without stimulating the least response. One is warranted, then, in concluding, with previous writers, that the mating of this species does not occur, normally, either on the ground or on some support.

While watching the females promenade to and fro upon the grapevine, numerous males and a few females flew away from the nest. When I had fully satisfied myself that mating occurred neither on the ground nor on a support, I arose and looked about me. For fifteen to twenty feet above the ground, the air was thick with the minute, gnat-like, males of this species. Not only the atmosphere above my yard, but that above all of the yards of the vicinity was alive with these miniature creatures, for the males of all of the *Lasius* nests of the city were having an aerial dance. They rose and fell, swerved to first one side and then to the other, occasionally they alighted on the ground, and, after a short rest, unless captured by the foraging ants of another genera, arose and repeated over and over again the maneuvers—thus they performed the prenuptial dance of the species, and all of the participants were males.

From time to time lone virgin females appeared in the midst of the dancing bachelors. Starting near the ground, such a female would corkscrew upwards, sometimes vertically, sometimes obliquely, and disappear above the two-story houses. Some of these females caused no disturbance of the dance; others attracted towards them one or more of the participants which accompanied them beyond the range of human eyes. Somewhere in the air mating would occur and the female, no longer a virgin, would return to earth. I did not have the pleasure of observing a pair at the moment of copulation; but I captured several of the brides as they descended; each with her miniature husband attached, appendage like, to the tip of her abdomen.

The altered appearance caused by the clinging of the male, made it easy to recognize a newly mated female afar off. Several were followed until they settled on the plants of my garden. After alighting and before the male had detached himself, the female, by means of vigorous strokes of her third pair of legs, would break off the wings of first one side and then of the other. These wings were for the honey-moon flight; since the females of this species mate but once in a life time, they were cast aside as useless encumbrances. Those wings were badges of virginity; now that she had become a matron, she discarded those emblems of maidenhood.

What a feast these marriage festivities furnished the insect-feeding ants of the community! Around the outskirts of each band of excited ants, *Formica* scouts were capturing the male stragglers and dragging them alive to their nests. All over the ground beneath the dancing males, active foragers of these same species of *Formica* were capturing such ants as happened to fall to the earth. Even the large females became prey of these alert ants. Often two and even three ants were observed dragging off the same female.

At the beginning of these observations, the sun was shining brightly; later the clouds became so thick that not a ray of sunlight could reach the earth. The prenuptial dance and the mating continued, in both sunshine and shadow, until about the close of day. Then the dancers gradually vanished until all that remained of the countless multitudes were a few straggling males and an occasional female. Even after the last of these males had disappeared, an occasional lone female would corkscrew upwards through the air. Poor belated virgins! Too late to perform the mission of their sex! Some, if not all, haunted the street lights of the city for a night or two; but the opportunity to become mothers had passed forever. The wedding festivities of *Lasius niger* had closed for the season. No new festivities could be inaugurated until, in some way not understood, the physiological and meteorological factors had stimulated a future generation of ant maidens and ant bachelors to wed.

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THE BEHAVIOR OF FUNDULUS, WITH ESPECIAL REFERENCE TO OVERLAND ESCAPE FROM TIDE-POOLS AND LOCOMOTION ON LAND.[1]

S. O. MAST

From the Zoological Laboratory of the Johns Hopkins University

It is difficult for one not familiar with life in the sea to realize what a fierce struggle for existence many of the smaller fishes have to wage. Our common minnows, e. g. Fundulus, are beset on every side with danger. They are continuously hunted from below by many predaceous fishes and from above by various sea-birds. For these creatures the price of existence is indeed eternal vigilance. Owing to this price, no doubt, they are among the most wary of fishes. The least disturbance in the water from below or merely an approaching shadow from above sends them scurrying for places of safety.

For purposes of protection they are usually found in shallow water very near the shore-line. As the tide rises they continuously follow the water inland keeping quite near the edge, and as it ebbs they follow it out again. On the newly covered bottom they are frequently seen rooting in the sand, apparently feeding. Thus the movement in harmony with the tide probably serves them in securing food as well as in protection against enemies. But in following the tide aquatic animals are also exposed to danger, for with the incoming tide they are often directed into depressions in the beach which are of such a nature as to hold water for a considerable time after the tide recedes but not until it rises again. To linger at the shore-line in these pools waiting for the water to recede would mean certain death to most aquatic animals. How does it happen that Fundulus, which is so frequently found in such pools, and ordinarily does remain at the water's edge is rarely if ever caught in them.[2] It was this question that inspired the following experimental observations, all of which were made during the summer of 1914 on a sand beach at Beaufort, N. C.

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[1] Published by permission of the Commissioner of Fisheries.

I shall first give in a general way the results of these observations, all of which were repeated a considerable number of times; then I shall present a few experiments in some detail.

If Fundulus gets into a tide-pool while the tide is rising it usually swims about in a deliberate sort of way, stopping here and there to root in the sand and to play with its companions. This behavior continues until the tide turns or at any rate until it is very nearly high. After that the animals may still swim about much as they did before, but they invariably, every few moments, return to the outlet of the pool and swim out and in again. Thus they continue to test the depth of the water in the outlet, and as soon as it gets too shallow they leave the pool and do not return. This accounts for the fact that they are not caught in these pools under ordinary circumstances.

But what interested me primarily was the behavior observed in pools in which the outlet had been closed before the fishes had escaped. Under such conditions it was found that the behavior depends very largely upon whether the water is running in or out of the pool at the time it is closed.

If the water is running in, nothing phenomenal occurs. The animals may swim about rather rapidly for a few moments, but even if they do, they very soon become quiet and proceed to feed and play in their accustomed manner. This experiment was repeated many times and only in one case was the behavior essentially different from that described, and in this case the tide was very nearly high at the time the pool was closed. The response observed under these conditions lead to some important conclusions that will be stated later in connection with a detailed description of the experiment.

If the water is running out when the pool is closed the behavior of the fish is quite different from what it is if the water is running in. After the pool is closed under these conditions, they first swim about rapidly in various directions for a few

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[2] I have again and again, during the ebbing tide, examined numerous tide-pools, but I have never found Fundulus in any of them after the water had stopped flowing out although in some instances they were still 40 to 50 meters long and contained water 20 to 30 cm. deep.

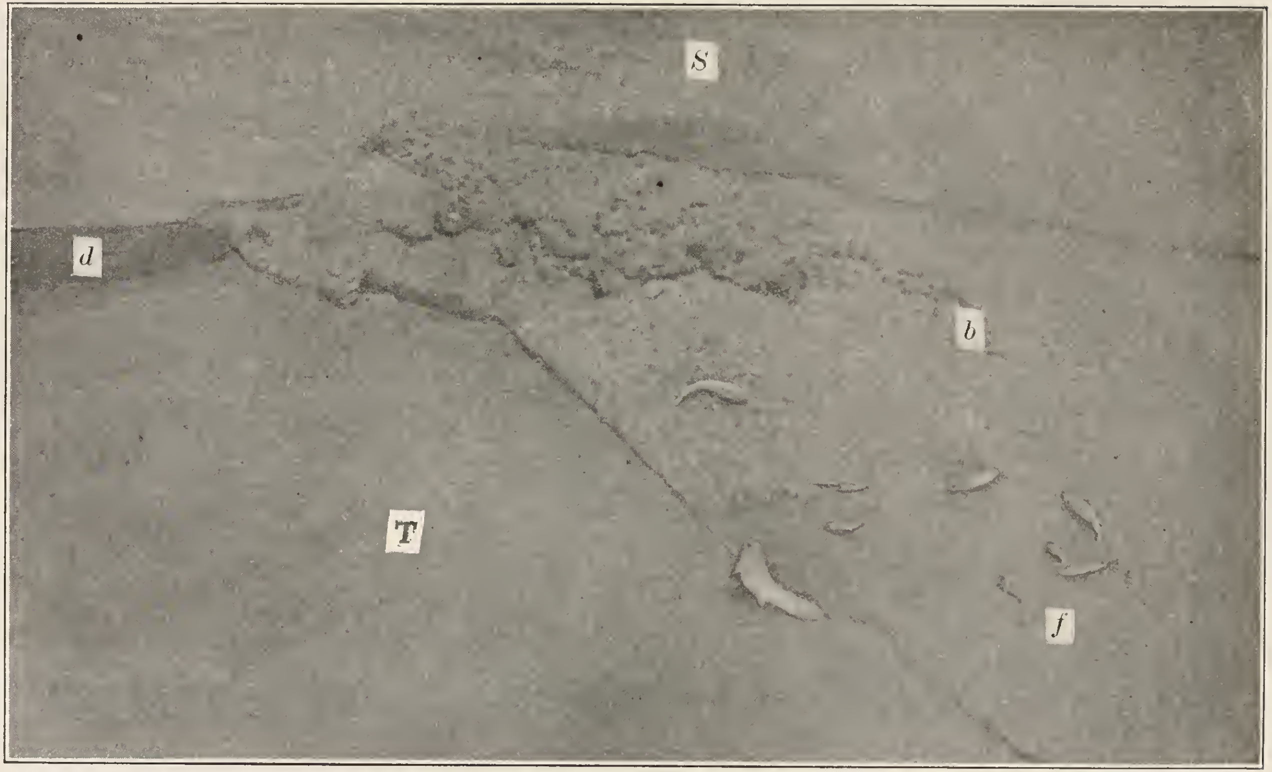

FIG. I. Instantaneous photograph showing a group of ten specimens of Fundulus shortly after they had left a tide-pool. They are traveling over a sand-bar about three meters wide to the sea. f, Fundulus majalis; T, tide-pool; d, dam; b, sand-bar; S, sea. Note that the animals were facing in all directions at the instant the photograph was taken. All were, however, proceeding toward the right.

I am indebted to my friend, W. P. Hay for this photograph.

moments, apparently very much excited; then they usually swim two or three times entirely around the pool keeping very close to the edge much as though they were looking for an outlet; after this a number of them ordinarily crowd together and wriggle well up on the beach into very shallow water. This usually occurs in the original outlet at either end of the dam across it. Finally one flops entirely out of the water onto the sand. Others follow immediately much as sheep follow a leader. After they have left the water they continue flopping and proceed directly across the bar which separates the pool from the sea. (See Fig. I.) Those that are left ordinarily swim about again for a few moments then collect as before, after which more

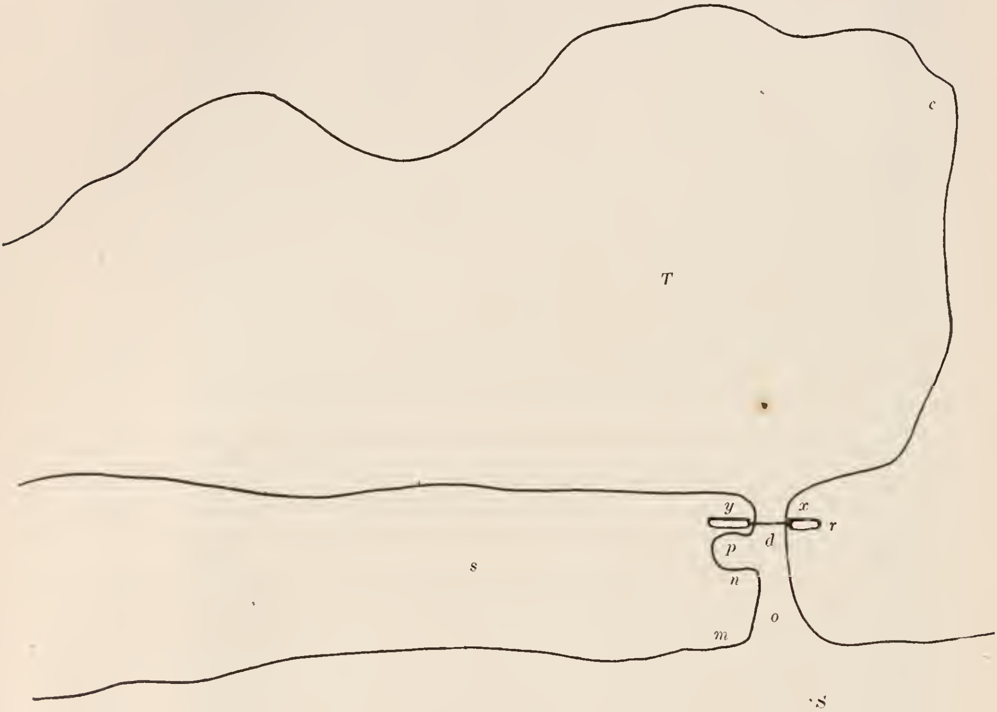

FIG. II. Outline of a portion of a tide-pool with the outlet closed by means of a board. This pool was 50 meters long, 13 meters wide and about 20 cm. deep. It contained approximately 300 specimens of Fundulus, all but 75 of which escaped to the sea by crossing a sand-bar 3 meters wide and fully 10 cm. high. T, tide-pool; o, outlet; S, sea; s, sand-bar; d, dam; r, ridge of sand; p, small pool; c, m, n, x, y, points mentioned in the description.

escape. This is repeated, one group following another, until all or nearly all have escaped.

In this way I have seen more than 200 of these fishes leave a tide-pool 50 meters long, 13 meters wide and 30 cm. deep, and travel across a sand-bar more than 3 meters wide and 10 cm. high, all in the course of half an hour. And I have seen them proceed in a fairly direct course toward the sea even against a moderately strong wind. I have also seen them persistently attempt, continuously for at least a minute, to go overland to the sea against a wind so strong that they could make no headway. When I first saw this performance I was deeply impressed. I had often seen fishes, when thrown on the land, flop back into the water in a more or less aimless fashion, but I had never seen any voluntarily leave a body of water and travel in a coordinate way on land. Concerning the nature of this phenomenon and the regulation in direction of locomotion on land I shall have something to say presently.

The description given above is based upon numerous experimental observations among which the following are typical.

1. On August 30 a tide-pool containing numerous specimens of Fundulus majalis was discovered on a sandy beach. This pool, somewhat irregular in outline, had a maximum length and width of 50 and 13 meters respectively and the water in much of it was, in places, more than 20 cm. deep. A strong current about 3 cm. deep was running out through the outlet, and some of the specimens were continuously passing out or in through it. At 5 P. M. all in the immediate neighborhood were driven in and then the outlet was suddenly closed with a board which extended 5 cm. above the surface of the water. A ridge of sand 60 cm. long and 10 cm. high was thrown up at either end of the board. This ridge extending 16 cm. above the water in the pool, joined on one side of the outlet, a natural bar of sand of the same elevation, so that the pool was separated from the sea on this side by a continuous barrier having an elevation of 15 cm. On the other side however, the natural bar had an elevation of only 5 cm. On this side in the angle between the ridge and the outlet, there was a considerable depression containing a small pool of water connected with the outlet, as represented in figure 2. The sand-bar was at every point over three meters wide. The bank at the edge of the pool

on the sea-side was everywhere very steep, and the water became deep rapidly. On the opposite side the incline was very gradual, and the water was very shallow.

As soon as the pool was closed the fishes began to swim about rapidly in an aimless sort of way. They continued for a few moments, then they came very close to the edge of the water and swam several times up and down the side of the pool nearest to the sea. Finally a dense aggregation formed in the outlet near the dam and soon, three minutes after the outlet was closed, they began to come out in the angle between the outlet and the dam represented by y in figure 2. The first group that left the water consisted of about twelve individuals. All of these followed the ridge from y to its end, and then turned and went toward the sea. Other groups soon followed behaving in a similar way. Many attempted repeatedly to cross the ridge at y and three actually succeeded although the ridge was fully 10 cm. high and the incline over it formed an angle with the horizontal of more than 45 degrees. After passing the ridge some went directly across the sand-bar and entered the sea at m, but many of them got into the small pool p. All of these swam directly across this pool to the bank at n. Here three were seen to leave the water again, climb the relatively steep bank 9 cm. high and then proceed to the sea, although this pool had a free passage to the outlet through which nearly all escaped. This seems to indicate that after these creatures once start in a given direction toward the sea they have a strong tendency to continue in this direction.

About 25 individuals were seen to leave the tide-pool at c, but all returned. A few of these reached a point nearly a meter from the edge of the water before they returned but most of them went only a few centimeters. Quite a number also left the pool at x but all of these returned after going a very short distance.

When the pool was closed there were approximately 300 specimens in it. The following morning 75 dead ones were found; consequently some 225 must have escaped.

2. In all of the experiments made during the falling tide behavior similar to that described above was observed. In some of them, there were, however, additional points of interest. A detailed description of one of these follows.

In this experiment a dam was thrown across a long narrow tide-pool running parallel with the coast-line as shown in figure 3. In this way approximately 150 specimens of Fundulus majalis of various sizes were enclosed. The sand-bar between the pool and the sea varied in height from 10 to 15 cm. This bar rose rapidly along the edge of the pool on the sea side, but on the opposite edge of the pool the inclines was very gradual: so that the elevation at the end of the dam on this side was only 3 cm., while on the sea side it was over 10 cm.

Observations were continued for 20 minutes. During this time nearly 50 specimens escaped by traveling overland around the end of the dam from x to y, i. e. on the land side where the elevation was least. These specimens were opposed in their locomotion on land by a fairly strong wind. A few escaped at the opposite end of the dam, going overland from m to n. And two crossed the sand-bar taking a direct course to the sea. A few also came out a short distance elsewhere but all of these returned to the pool.

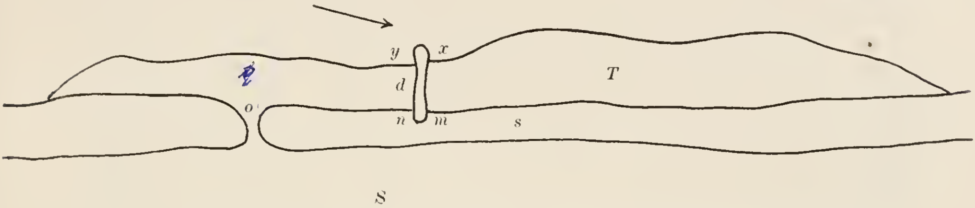

FIG. III. Outline of a long, narrow tide-pool with a dam thrown across near the middle. This pool had a maximum width and length of 2 and 24 meters respectively. It contained approximately 150 funduli, most of which escaped by traveling overland, against a moderately strong wind, around the end of the dam on the land side, xy. T, tide-pool; o, outlet; S, sea; s, sand-bar; d, dam; m, n, x, y, points mentioned in the description. The arrow indicates the direction of the wind.

The results obtained in these two experiments and others show that Fundulus tends to leave the tide-pools near the original outlet. Relatively few were seen to attempt to escape elsewhere in spite of the fact that the incline of the bottom was usually much more gradual in many other places. They also show that there is a tendency to select the lowest place near the outlet. This is particularly evident in experiment 2. Ordinarily these creatures leave the pools on the side of the outlet nearest the sea but in this case they left on the side nearest the land where the elevation was much less than on the opposite side.

The results show, moreover that after the fishes are out on the land they tend to go directly toward the sea. This is evident from the persistent attempts made in experiment 1, to cross the ridge at y, and from the direct course taken after passing the ridge, especially in the small pool p. They show, furthermore, that the tendency to go toward the sea is not a response to the light reflected from the water, for in experiment 1, the fishes, when they were behind the ridge, persistently attempted to go toward the sea, although in this position, the ridge effectively hid the sea from view while the pool was fully exposed. In experiment 2 they also proceeded toward the sea under similar conditions, or rather toward the original outlet of the pool.

3. As previously stated, if the tide flows in when the pool is closed, nothing out of the ordinary occurs in the reactions of Fundulus. Only in one experiment, that described below. was there an exception to this. Unfortunately, owing to other duties, I was unable to repeat this experiment under the same conditions.

On September 7, at 10.19 A. M. the outlet of a large tide-pool (12 by 30 m.) containing about 350 funduli was closed. At this time there was a strong current of water running into the pool indicating that the tide was still rising. Immediately after, the pool was closed, the fishes began to swim about rapidly being apparently very much excited, and two minutes later they began to come out of the water. In short, they behaved precisely as they ordinarily do when the tide is running out, not at all as they ordinarily do when it is running in. They continued to come out for some time, most of them, as usual near the original outlet, but nearly all of them returned to the pool; only a few succeeded in crossing the sand-bar which separated the pool from the sea. The sun was very hot at this time and the sand on the bar rather dry. This probably accounts for the fact that nearly all returned to the pool after proceeding a short distance toward the sea. At 10.40 the tide had unquestionably turned for the water outside the pool was already several centimeters lower than that inside. The tide was consequently very nearly high when the pool was first closed and this no doubt, was the cause of the unusual behavior. If this is true it must be assumed that in some way these animals know when the tide is about to turn, for their method of response changes from that characteristic of the rising tide to that characteristic of the falling tide before the tide turns.

At 11.10, i. e. nearly an hour after the pool was closed, the fishes were much more quiet than they had been earlier, Most of them were swimming about in a leisurely fashion, some were feeding and none were coming out of the water. They were observed for some time after this, but at no time was there the slightest indication of an attempt to leave the water, although various methods were used in trying to make them leave, e. g., boards were thrown into the pool, the water was violently disturbed by running around in it and much of it was drawn off. Later the water in this pool together with the fishes was drained into a lower pool. In this pool the fishes swam about rapidly as though they were considerably excited but none of them left the pool, although a few at different times came out of the water a short distance. Their behavior in general was markedly different from that observed in animals suddenly shut in pools during the ebbing tide . This indicates strongly that the all-important factor involved in the behavior resulting in the overland escape of Fundulus from tide-pools is the sudden closing of the outlet through which it is accustomed to go. The location of this outlet they evidently remember for some time. The results of this experiment show also that Fundulus becomes very rapidly acclimated.

The movement of these fishes on land seems to be well coordinated. They travel in fairly direct courses. There is nothing in the nature of aimless tumbling about as is ordinarily seen in the behavior of fishes out of water. Locomotion consists of successive leaps due to sudden bending of the body. When the fish falls after a leap it may be directed toward any point of the compass, but the succeeding leap carries it on its course no matter in which direction it may be facing at the time of the response. Thus before each leap it may be headed in the direction in which it is traveling or in the opposite direction or in any other direction. It is really remarkable that the bending of the body is so regulated that the animal continues to move in a given direction regardless of its axial position at the beginning of the successive reactions. As to the mechanics of the process I am as yet quite in the dark. And I am also unable to say what factors in the environment serve to direct these animals overland to the sea. Vision of the sea seems to play little or no part in this, for the fishes continue toward the sea if a screen is so placed that the water can not be seen; or if conditions are so arranged that the largest surface of water visible is in the tide-pool. The slope of the beach can also not serve to guide them, for in crossing the sand-bar they have to go up grade as well as down. Nor are there any other external features that seem capable of serving as a guide. The phenomenon is consequently probably very largely dependent upon internal factors.

SUMMARY

1. Fundulus is frequently found in temporary tide-pools, but rarely if ever after the water is so low that the outlet is closed. When the tide is falling it swims out and in at short intervals but as soon as the water in the outlet gets low it does not return. In this way it avoids being caught in these pools and killed when they dry during low tide.

2. If the outlet is closed while the tide is rising nothing out of the ordinary occurs, but if it is closed while the tide is falling the fishes swim about rapidly in various directions for a few moments. Then they come out of the water and travel overland to the sea. Many specimens have been seen thus to leave large tide-pools and travel across sand-bars more than 3 m. wide and 10 cm. high.

3. Fundulus nearly always leaves the pools on the sea side near the original outlet. It apparently remembers the location of the outlet; and it is the sudden closing of this that constitutes the principal factor causing these fishes to leave the pools.

4. On land they never travel in the wrong direction any considerable distance. It is not known how they are guided in the right direction, but it is known that light reflected from the water is not a significant factor in the process.

5. Locomotion on land is brought about by successive leaps due to rapid bending of the body. The course taken is fairly direct. Every leap carries the animal in the right direction, although the axial position at the beginning of the successive leaps varies greatly; the fish, at this time, may be headed in the direction of locomotion or in the opposite direction or in any other direction. The movements appear to be well coordinated, but the process involved in thus regulating the direction of locomotion is not understood.

EXPERIMENTS ON SEX RECOGNITION AND THE PROBLEM OF SEXUAL SELECTION IN DROSOPHILA

A. H. STURTEVANT
Columbia University

Much has been written on the subject of sexual selection since Darwin first developed the theory, and many remarkable observations have been recorded. There has, however, been very little experimental work in this field. Darwin and those who have followed him have obtained much of their evidence from the insects, and within this group some of the most striking cases of elaborate mating habits have been reported in the Diptera, and here too there is to be found a most remarkable array of secondary sexual characters. Perhaps the most extreme case is that of the *Elaphomyia* described by Wallace, in which the male has long, hornlike processes arising from his head, which are absent in the female. The families of Platypezidae, Dolichopodidae, and Empididae are especially rich in secondary sexual characters, which occur in the legs, wings, antennae, face, or other parts. In the two latter families some very curious observations on mating habits have been recorded (see especially Poulton's account ('13) of Hamm's work on Empididae).[1]

Some of the best experimental evidence in favor of sexual selection is that obtained by Lutz (1911), who worked with an abnormal wing venation in *Drosophila ampelophila* which he found to be strongly selected against. To Dr. Lutz is due the suggestion that the mutants in this fly obtained by Morgan would form excellent material for the study of the problem. The method of some of the experiments was also suggested by Lutz. I took up the matter at the suggestion of Professor Morgan, to whom I am greatly indebted for most of the material and for his encouragement and criticisms. Several discussions of the matter with Dr. Lutz have also been very helpful. Some of the work was done at the Cold Spring Harbor Laboratory in the summer of 1911, and I wish to express my appreciation of the interest in the experiment shown by Dr. C. B. Davenport at that time.

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[1] I have myself observed courtship in a few Dolichopodidae and in a number of other Diptera. These observations will be published in full later.

COURTSHIP AND MATING

Most of my observations on courtship in *Drosophila* have been made upon *D. ampelophila* Loew. In general the process is very similar in *D. busckii* Coq., but differs in several respects in *D. amoena* Loew, *D. repleta* Woll., and *D. funebris* Fabr. The first and most noticeable act in courtship occurs when the male, being near the female, extends one wing at about right angles to his body, and vibrates it for a few seconds. The wing is then returned to the normal position and the process is repeated, usually with the other wing. But between times there is a scissors-like movement of the wings repeated several times. This vibrating of the wings is often repeated many times, and may be done in any position relative to the female, though the male always faces her. Usually, in fact, he swings quickly around her in a semicircle once or oftener during the process. Soon the male begins to protrude his genitalia and, if the female remains quiet, to lick her posterior end. Some white matter now protrudes from her ovipositor, and other males in the same vial are usually observed to become excited now and begin courting; indicating odor as a cause of sexual excitement. If the female runs or flies away the male is excited, moves his wings jerkily, and walks around rapidly, but seems unable to follow the female accurately or to locate her quickly. The penis is directed forward by bending up the abdomen underneath, towards the thorax, and is jerked toward the female (the male always standing facing her at this stage), but not always toward her genitalia, as I have seen it strike her in the eye.[2] If it does strike the mark the male mounts on the female's back, between her wings. Mounting never takes place until after the actual copulation has occurred, in which respect *Drosophila* differs from some related flies (e.g., Muscidae, Anthomyidae, Sepsidae, Borboridae, and Ephydridae, so far as my observations go). In these forms the male flies and lights on the female, after which copulation may or may not take place, probably depending upon the way the female responds.

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[2] The male in this case, however, had white eyes, and so was perhaps blind, Normally the aim is accurate.

Berlese ('02) and Hewett ('08) find that in the house fly the final step in copulation is taken by the female, which inserts the ovipositor into the genital opening of the male. I have not been able to verify this for *Drosophila*, but it is probably true here also. In *Drosophila*, as in some other related flies (I have examined a few Anthomyids and *Sepsis violacea*), the ovipositor enters the male opening, instead of the penis entering the female duct. But I cannot state positively that the female inserts it instead of the male drawing it in by means of his genital armature. In any case, it is certain that the female is not entirely a passive agent. The time required for copulation to take place depends largely upon whether she stands quietly and allows the male to pair, or moves away when he begins to court. In the latter case very active males have been seen to pair while the female was walking away, but this is exceptional. Occasionally a female seems to frighten off a male by spreading her wings and moving quickly toward him.[3] When this happens he moves off, and does not so far as I have seen, then pair with that female, although she has been known to pair with another male within a few minutes afterward. That this is really a threat on the part of the female seems likely from observations of fighting between males. If two males are courting the same female they often grow very excited, especially if she is unwilling to stay quiet. In such cases they may sometimes be seen to spread their wings, run at each other, and apparently butt heads. One of them soon gives up and runs away. If the other then runs at him again within the next few minutes he usually makes off without showing fight.

The time occupied by the process of courtship varies greatly with the age and condition of the flies and with the temperature. Copulation may occur within a few seconds after the flies are put together, with little preliminary courtship. While experimenting with flies about 3 or 4 days old, which had never been allowed to pair, I have found that 20 minutes or a little less is about the average length of time before copulation occurs. The flies may remain *in copula* for only a few seconds, but so far as my observation goes this occurs only after a prolonged courtship in which the female has seemed unwilling to mate, and is, I think, due to the male not getting a good hold. I do not know whether or not such pairings are successful. Ordinarily copulation lasts about 20 minutes.[4]

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[3] Compare Howard's ('02, pp. 141 and 145) accounts of Asilid and Empidid flies eating males which were courting them.

EXPERIMENTS ON OTHER INSECTS

There is a considerable body of evidence relating to the question of sex recognition in the Arthropods. A short review of the subject and a bibliography are given by Chidester ('11), so I shall confine myself here to the evidence dealing with insects. There is evidence from several groups of insects, but most of it points the same way. Sex recognition at a distance is by smell, but the actual process of copulation depends upon the sense of touch.

In the Orthoptera, Stockard ('08) has observed the male walking stick to pair with the detached abdomen of a female which was fastened to a stick with wires for legs.

In Coleoptera, Féré ('98) finds that male cockchafers do not pair if the antennae are removed. Males will sometimes pair with males, provided the latter have just paired with females, or have been artificially impregnated with female juices.

Mast ('12) shows that in the firefly the males find the females by means of the flashing lights, signals being made by both sexes. Tower ('06) reports that in *Leptinotarsa* males normally never try to actually copulate with males, but that if the antennae are removed or painted with shellac they will try to pair with any individual they happen to touch. If this individual is a female pairing will occur. If the abdomens of females are removed the males are attracted by them, though not by the wings, head and thorax. Females with their abdomens coated with shellac are not attractive to the males.

Féré ('98a) found that in the silkworm moth males will pair with other males, which have not been given a chance to get the female odor, if the latter males are sluggish, as after removal of the antennae. Males without antennae will copulate. Kellogg ('07) has since reported that males without antennae find the females only by chance, while normal males go straight to them. If one antenna be removed the male travels in a circular path instead of going toward the female. If a male without antennae happens to touch a female he immediately shows strong sexual excitement, such as normal males show when brought near females. Scent glands on the abdomen of the female were shown to be the seat of the olfactory attraction, for when the abdomens were removed males were attracted by them and not by the rest of the female. Blackening the eyes produced no change in the behavior.

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[4] In 20 cases the duration of copulation was timed with the following result (minutes): $\frac{1}{2}$, 5, 16, 17, 18, 18, 19, 20, 21. 21, 21. 21, 22, 22, 23, 24, 25, 26. 27. 33,

Mayer (1900) performed numerous experiments on the moth *Callosamia Promethea.* He showed that it is odor that attracts the males, and that this odor comes from the abdomen of the female. Interchanging of wings indicated that the marked sexual dimorphism in color, which occurs in this species, has no selective value.

Kirkland (1896)[5] showed that in the Gypsy moth odor is again the main element concerned, but the wings of the female, as well as her abdomen, have an exciting odor. Mayer and Soule (1906) tried experiments on this form, which they supposed indicated that normal females discriminated against males without wings. I find, by applying Yule's (1911) formula for the standard error of the difference, that the difference between the per cent of times winged males paired without resistance and the per cent that wingless males paired without resistance (the measure of sexual selection used by these authors) is almost exactly three times the standard error. This means that the result is not conclusive. Mayer and Soule blinded females and found this apparent discrimation against wingless males to disappear, but the per cent of resistance dropped, as a whole, from 46% in the normal females to 27% in the blinded ones. This may mean that the blinded ones were too greatly disturbed by the blinding to pay much attention to what male mated with them. While I am inclined to suspect that there is some odor connected with the male's wings, still, as stated above, it is not certain that any effect at all is produced by removing the wings of the male. Further evidence against the importance of sight is furnished by the fact that females did not discriminate against males with wings painted in unusual colors.

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[5] I have not seen this paper, but make the statement on the authority of Mayer and Soule (1906).

Federley (1911) gives evidence indicating that odor is a strong sexual stimulator in the moth *Pygaera*, and may even cause males to attempt to pair with the bars of the cage in which they are confined. Entomological literature contains many accounts of the remarkable ability of male moths to locate females though smell. A few of the more striking cases are given by Washburn ('09, pp. 87 and 88.)

SEX RECOGNITION IN DROSOPHILA

It was noted above that when an egg protrudes from the ovipositor of a female any males in the same bottle usually become excited and begin courting. When a female is killed eggs are likely to protrude, and males are often seen vigorously courting females which have been freshly killed, even though the killing agent were so strong smelling a substance as ether. In such cases it is the posterior end of the abdomen which seems to be the chief focus of attraction. The attraction may last for at least 30 minutes after death, and probably longer. I have not as yet been able to cause males to copulate with dead females, probably because the female normally takes an active part, as indicated above.

If a male which has just paired is placed with a male which has been isolated from females for several days he may be courted by that male, though this is not frequent. If two males are kept in solitary confinement for four or five days and are then put together they often court each other, in one case even the doubling up of the abdomen being seen, and the wing movement (*not* fighting) being almost invariably observed. Such mutual courtship between males has not been seen under other conditions, but a dead male which had not been with females for two days before killing has been seen to be courted by other males. This courting, however, seems more likely to occur when juices from female abdomens are put on the dead males. In *Psychoda* sp. courtship of males by males seems to be very frequent, and often copulation is attempted. The genitalia even become attached, and stay so for several seconds. Males have been seen to mount males in *Fucellia marina*, *Sepsis violacea*, and *Sarcophaga* sp.

In order to test what part is played by the female juices mentioned above I placed some on bits of filter paper and put these in the same vial with males. The males paid no attention to the paper, and showed no signs of excitement, though that they were sexually ripe was shown by the fact that they courted and paired with females when they were put in with them. This experiment has been repeated several times with the same result. When a female is thoroughly crushed and the remains heaped up in a little pile the males will sometimes court slightly, but as a general rule, the more mashed the female is, the less excited the males become. No great importance is to be placed upon this latter experiment, as the result is complicated by the presence of so many other body juices. This objection, however, will hardly hold in the case of the filter paper impregnated with female juices. The objection that not enough odor was present may perhaps be justifiable in that case. But neither of these objections would seem to apply to the following experiment. Two females were placed in a small, dark, cloth bag and this was put in a vial with a few males. The males were close to the females, which they could not see or touch, but should be able to smell. This experiment was done three times and in no case did the males show any signs of sexual excitement, though in all three cases they did court immediately afterwards when given females in the usual way.

It has been shown by Barrows ('07) that the sense organs for smell in *Drosophila*, insofar as one may judge from reactions to food substances, are located in the terminal antennal joints. For this reason I was led to perform the following experiments, in an effort to determine the part played by the sense of smell in courtship and copulation.

The antennae were removed from several males,[6] which were after several days placed with virgin females. Such males are very sluggish, and it was therefore not surprising that no courtship was observed. However, one such male was found copulating, and at least three of them left offspring after the operation. One of the three had white eyes, and was therefore probably also blind. Courtship has been observed in two males from which the antennal aristae had been removed. A normal male has been seen copulating with a female from which the antennae had been removed, and this pairing resulted in the production of offspring.

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[6] I have found that antennae may easily be removed without using the complex method described by Barrows, if the operation be performed on very young flies.

The above experiments failed to give any evidence demonstrating that smell alone can cause males to show signs of courtship. Another series of experiments, now to be described, has indicated, however, that smell may be a secondary factor in causing sexual excitement. Males and virgin females were isolated for three or four days and were then placed, in pairs, in clean vials. The length of time before copulation occurred was recorded; and, after it had taken place, the flies were removed and a new pair was placed in the same vial. The only difference between the two sets was that one lot was in clean vials, the other was in vials in which copulation had just occurred. In these experiments an equal number of each sort was done on each day (placing a third pair in some vials when necessary in order to get an equal number), and all flies used on a given day were as nearly the same age and size as practicable, and were from the same culture. Table I gives the result of the experiments. It shows that in the first five or six minutes the flies in the used vials are more likely to copulate than are those in the clean vials; between ten and twenty minutes there are more copulations in the clean vials, and after that the two series are parallel. Apparently there are a certain number of pairs that are nearly ready to mate, and these will mate more quickly if another copulation has just occurred in the same vial. But if much courtship is to be required before copu-

TABLE I

Minutes before Copulation	Number of times observed	
	1st pair	2nd pair
1– 3	13	22
4– 6	12	22
7– 9	5	7
10–12	7	6
13–15	11	2
16–18	7	5
19–21	8	5
22–24	4	3
25–27	5	3
28–30	2	1
31–33	7	1
34–36	2	2
37–39	0	2
40 and more, including failures	24	25

tation, then the effect of smell is negligible. It is not clear from these results whether the effect is produced upon the males, the females, or both.

In order to test whether sight is the sense which stimulates the male I carried out the following experiment. Two vials having the same bore were filled with cotton up to about three centimeters from the mouths, and then placed with their mouths together. A thin glass cover-slip was then placed between them, and a male fly was placed in one vial, a female in the other. These flies had been isolated for several days, and were sexually ripe. The male could see the female, but could not touch or smell her. Once or twice they met "head on" with only the cover-slip between them, but the male showed no signs of recognition. After thirty minutes the female was let in the vial containing the male; and the vials were left in position. He courted her within two minutes, and paired in five minutes. The experiment was repeated with the same result, except that copulation now occurred in three minutes after putting the flies together.

It is also evident that sight is not necessary for pairing from the facts that Drosophila breeds freely in the dark (see Payne, '11), and that pure stock of white-eyed flies (which may be blind) has been kept for many generations.

In *Fucellia marina* and in *Musca domestica* males seem to see their mates from some distance and fly directly to them, lighting on the back of the mate. Here sight would seem to play considerable part, and in *Fucellia* still more convincing evidence was obtained from observation of violent courtship of a fly which was separated by thick glass from the courter. Not only did this male court the other fly, but when she walked around rather rapidly, he very accurately followed her on the other side of the glass. This has been observed twice.

Since the wings play such a conspicuous part in courtship I was led to try the effect of cutting them off. Two males of the same age were used, the wings of one being cut off at the base. These were kept in the same vial for several days, and then placed with a virgin female several days old. The vial was then watched until copulation took place. This has been done 125 times, using different individuals each time. The normal male paired 72 times, the clipped male 53 times. As in all the similar experiments here reported care was taken to have the competing flies as nearly the same age and size as possible. The result indicates that there is very slight, if any, selection against the clipped males. It seemed possible that courtship made the female ready to copulate, but that she would then mate with either male. To test this hypothesis another series of experiments was carried out. A single pair of flies was placed in each vial, using each day an equal number of normal and of clipped males. The length of time before copulation was recorded in each case. A preliminary account of this experiment has been published by Morgan ('13); but the data used there have been discarded, since two serious sources of error had not then been recognized. It was not realized that the time before copulation might be influenced by a previous copulation having occurred in the same vial; and sufficient precautions against drying were not taken—a very important factor. A new series of experiments, in which these points were controlled, gave the results shown in table II. As a matter of fact, however, these data are very similar to the discarded series.

TABLE II

Minutes before Copulation	Number of times observed	
	Normal ♂	Clipped ♂
0– 3	15	4
4– 6	19	9
7– 9	10	7
10–12	15	5
13–15	3	7
16–18	8	4
19–21	3	10
22–24	3	4
25–27	9	4
28–30	2	2
31–33	2	2
34–36	0	4
37–39	1	2
40–42	3	2
43–45	2	1
46 and over, including failures	43	73

This table seems to justify the suspicion that led to the experiment. Had the females discriminated against the clipped males to the extent shown above when both kinds were present, the normal males would certainly have appeared at a greater advantage. That the result was not due to less activity on the part of the clipped males is indicated both by the contests described above and by the following observations. In some of the experiments recorded in table II the number of minutes before courtship began was observed. Table III indicates that the clipped males began courting as quickly as did the normals.

TABLE III

Minutes before Courtship	Normal	Clipped
1– 2	12	9
3– 4	2	3
5– 6	1	2
7– 8	1	2
9–10	0	1
11–12	1	0
13 and over	7	7

From these experiments it seems certain that the wings are of value in courtship; but the effect probably is to produce sexual excitement in the female, rather than to cause her to select a male that uses his wings. No " choice " is involved; but, as pointed out by Watson ('14, p. 173), the effect, in nature, would be strongly in favor of the normal male.

It seems probable that touch is of considerable importance in the sexual process, and all my observations are consistent with that view, but I have no direct experimental evidence to that effect. It is not possible to get evidence from *Drosophila* such as Kellogg obtained from *Bombyx*, because the flies are more active, and less easily sexually excited, than is the silkworm moth.

Two kinds of experiments have been carried out in an effort to find out what part of the body is responsible for causing sexual excitement. A female without an abdomen, but alive and active, was placed with males that had been isolated from other females for four days. She was vigorously courted.

Three gynandromorphs have been tested to determine their sexual behavior. None showed any certain indications of male behavior, but all were vigorously courted by males. Of these three gynandromorphs the external characters were as follows: (A) All female, except one side of the head, which was male; (B) female on one side of the whole body, male on the other side; (C) female, except the genitalia, which were male. It is doubtful what conclusion, if any, is to be drawn from these few observations.

SEXUAL SELECTION—ARTIFICIAL ABNORMALITIES

The male of *Drosophila ampelophila* bears a small comb on his front metatarsi, a secondary sexual character not found in most species of the genus.[7] Lutz ('11) has shown that the removal of this comb has no effect upon the availability of males for copulation. That is, the females were not influenced by the sex-comb in their choice of mates.

It sometimes seems to be difficult for the male to get the wings of the female out of his way so that he can mount her. For this reason I carried out an experiment the converse of one recorded above; using two virgin females of the same age, one with normal wings, the other with wings removed. These were put with a normal male. In 52 successful trials the normal female was paired with 25 times, the clipped one 27. Again it would seem as though clipping the wings has very little if any effect.

SEXUAL SELECTION—MUTANTS

Lutz ('11) found that certain slight abnormalities in wing venation were selected against both by normal and abnormal flies of both sexes. The abnormal flies were not noticeably different from the normals in behavior, so that it seems quite unlikely that the results were due to a difference in the activity of the two types. Moreover, if, let us say, the normal male was more active than the abnormal, so that he would be more likely to pair first, it would seem that the normal female would not be so easily paired with as the abnormal; or *vice versa*, if we suppose the abnormal female to be less active and therefore more likely to be paired with, it is hard to see why the abnormal male should be at a disadvantage. Further evidence bearing out this view of the effect of differences in activity will be given below. Lutz suggested that it seems unlikely that sight could have any influence, and that perhaps there is some unpleasant smell correlated with the abnormality, this being the basis of selection.

I have conducted a series of experiments upon some of Morgan's *Drosophila* mutants, in an effort to find if there was any selective mating in connection with them. The following mutants were used:

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[7] So far as my observation goes it is present only in *D. ampelophila*, *D. confusa*, *D. obscura*, and three or four undescribed species.

(1) White eyes. This form was first described by Morgan ('10). There is no color in the eye. This probably means that the fly is blind, and it has often been observed to be less strongly phototactic than the wild fly. White eyed flies are also less active and vigorous than normals.

(2) Yellow body color. Described by Morgan ('11). The whole body of this fly is lighter than that of the normals, and there is a distinct yellow color to the wings. Sight seems to be normal, but again the flies are not so active as are the normals.

(3) Curved wings. The main interest of this form in this connection is that the wings are always held extended, in somewhat the same position as that of the courting males. The flies are perhaps a little less active than the normals

(4) Vermilion eyes. Described first by Morgan ('11a). These flies differ from the normals in that the eyes are of a brighter and less intense red. Their vigor and activity is very little, if at all, inferior to that of the normals.

Some of the flies used had yellow bodies and white eyes, and in one experiment a few vermilion eyed yellow black colored females were used. This latter combination was at the time considered one of the weakest stocks in the laboratory, and was used for that reason.

The method of the experiment was as described above for the wing clipping experiment. For instance, a white eyed female would be given her choice between a red male and a white one of the same age and size. Then the experimenter simply watched until pairing was seen, and the flies were then thrown away. The following table (IV) gives the results obtained.

TABLE IV

Red vs. white eyes. (Normal body color.)

"Chooser"	"Chosen"	Number of cases
Red ♂	Red ♀	54
	White ♀	82
White ♂	Red ♀	40
	White ♀	93
Red ♀	Red ♂	53
	White ♂	14
White ♀	Red ♂	62
	White ♂	19

TABLE IV—*Continued*

Gray (normal) vs. yellow body color. (Red eyes.)

Gray ♂	Gray ♀	25
	Yellow ♀	31
Yellow ♂	Gray ♀	12
	Yellow ♀	30
Gray ♀	Gray ♂	60
	Yellow ♂	12
Yellow ♀	Gray ♂	25
	Yellow ♂	8
Vermilion Black-yellow ♀	Red-gray ♂	13
	White-gray ♂	1

Gray and yellow body colors. (White eyes.)

Gray ♂	Gray ♀	11
	Yellow ♀	4
Gray ♀	Gray ♂	21
	Yellow ♂	3

Red and white eyes. (Yellow body color.)

Red ♂	Red ♀	3
	White ♀	4
White ♂	Red ♀	9
	White ♀	9
Red ♀	Red ♂	9
	White ♂	2
White ♀	Red ♂	21
	White ♂	1

Red and vermilion eyes. (Gray body color.)

Red ♂	Red ♀	7
	Vermilion ♀	5
Vermilion ♂	Red ♀	4
	Vermilion ♀	4
Red ♀	Red ♂	11
	Vermilion ♂	14

Long and curved wings. (Other characters normal.)

Long ♂	Long ♀	10
	Curved ♀	13
Curved ♂	Long ♀	5
	Curved ♀	3
Long ♀	Long ♂	14
	Curved ♂	4
Curved ♀	Long ♂	9
	Curved ♂	5

It appears that sexual selection is not involved in any of these cases. The impression gained from observation of over 1,000 " contests " of this sort is that the outcome is not a matter of choice. A female, in the great majority of cases, seems to allow the first active amorous male that comes along to pair with her; or, if she is disinclined to mate, resists all males apparently indifferently. When a male is sexually excited he pairs with the first female he finds which will allow him to. As a result, the more active and vigorous males are likely to win their contests, and the greater the difference in vigor, the greater the proportion of times the better male wins. This is just what the results show to happen. So far as the small numbers show, the vermilion male is at no disadvantage and the curved male is not quite able to hold his own, while it is certain that the yellow and white are far behind their normal opponents.

In the converse case, a less vigorous female will be less likely to resist or escape from the male successfully if she be disinclined to mate. But this influence should have less effect on the result than the one discussed above, since both females will often be willing to mate and will not try to escape. This again agrees exactly with the facts as given above.

The two results of unequal vigor discussed above seem to me quite adequate to explain all the results obtained. There is no evidence of any " choice " on the part either of males or of females. Unlike the abnormal venation studied by Lutz, these mutants probably have no significance from the point of view of sexual selection, in the narrower sense of that term.

SUMMARY

Experiments indicate that sight is not essential in sex recognition in *Drosophlia*. The olfactory and tactile senses are probably both concerned, as in most other insects.

The wings of the male play a conspicuous part in normal courtship. Experiments with males from which the wings had been removed indicated that the function of these organs in courtship is the production of sexual excitement in the female.

Experiments were carried out with four mutants (white eyes, vermilion eyes, yellow body color, and curved wings), involving the observation of 839 contested matings.

As a result of these experiments it seems probable that the

four characters involved have no selective value, except in so far as results from the fact that at least two of these classes are less active then normals.

In general it is probable that, in *Drosophila*, neither sex exercises any "choice" in the selection of a mate. A female that is ready to mate will accept any male, and a male that is ready to mate will do so with the first female that will allow him to.

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THE WHITE RAT AND THE MAZE PROBLEM

IV. THE NUMBER AND DISTRIBUTION OF ERRORS—A COMPARATIVE STUDY

STELLA B. VINCENT
Chicago Normal College

What is the maze problem? It is the learning of a difficult path, having many blind alleys, under a stimulus so strong and certain that finally, when put into the labyrinth, the animal runs swiftly and surely to the goal without error. It consists of a series of movements which tend toward a definite end and which are so ordered that the position and the direction of the turns seem to be the all important factors. The animal's task is to learn this motor co-ordination, ours, if possible, to find out how it does it.

In the learning of the maze we find not a single problem but a complex of many and much light has been thrown upon them through the work of Small, Watson, Richardson, Carr and others. Some of the questions which arise in the course of such investigations are, however, still without satisfactory answers. The preceding numbers of this series have dealt with the problems of sensory control in the maze. This paper attempts to deal, briefly, with some of the more general features of the maze problem in the light of that experimentation. One question which immediately suggested itself concerned the relative value of the different senses when directive in such a problem.

THE RELATIVE EFFECTIVENESS OF THE DIFFERENT SENSES AS MODES OF CONTROL

This comparison is not an easy one to make since any one sense factor is never isolated but only emphasized in the sensory complex. The following table (I) shows factual data taken in different ways from the learning scores. If we turn this into terms of per cent, making the lowest and therefore best score always 100, and basing the others on this we get table II. By normal maze is meant the unpainted wooden maze where the

TABLE I

	Time of learning	Average errors first 5 trials	Average errors last 5 trials	Total average errors per animal	Average time first 5 trials	Average time last 5 trials	Surplus time
Normal maze...	12.1 ±3.6 trial	48.7 ±7.7	.1 ±.14	66.6±16.	16.3±6.7 min.	.31±.05 min.	93.9±16.5 min.
Olfactory maze, trail in true path.........	8.1 ±2.4 trial	13.2 ±2.9	.04±.04	20.5±5.6	8.7±3.9 min.	.28±.08 min.	64.9±23.6 min.
Composite black-white maze.........	14. ±11. trial	17.4 ±4.5	.3 ±.4	34.6 ±8.4	7.6±4. min.	.4 ±.1 min.	47. ±16.8 min.
Cutaneous maze (sides on)	16.5 ±3.7 trial	30. ±12.8	.18±.21	51. ±23.	1.8±2.7 min.	.18±.06 min.	15.3 ±6. min.
Cutaneous maze (open).......	16.5 ±6.6 trial	21. ±5.2	.09±.11	37.3 ±5.1	1.9 ±.7 min.	.15±.03 min.	13.9 ±4.3 min.

TABLE II

Comparison of Normal, Black-white and Olfactory Mazes

Time of learning
    Olfactory 100%, Normal 145%, Black-white 175%

Accuracy
    Initial—Olfactory 100%, Black-white 130%, Normal 370%
    Final— Olfactory 100%, Normal 250%, Black-white 750%
    Total— Olfactory 100%, Black-white 175%, Normal 330%

Time
    Initial— Black-white 100%, Olfactory 115%, Normal 215%
    Final— Olfactory 100%, Normal 110%, Black-white 143%
    Surplus—Black-white 100%, Olfactory 138%, Normal 190%

Comparison of Open Maze with Enclosed Normal Maze of Same Pattern

Time of learning
    Open maze 100%, Normal 100%

Accuracy
    Initial—Open maze 100%, Normal 150%
    Final— Open maze 100%, Normal 225%
    Total— Open maze 100%, Normal 140%

Time
    Initial—Normal 100%, Open maze 105%
    Final— Open maze 100%, Normal 120%
    Total— Open maze 100%, Normal 110%

sides to the alleys were high enough to prevent any outlook to the neighboring runways and the light and odor were as evenly distributed as possible. The second maze was of the same plan but the true and the false paths were made to differ decidedly in brightness values. The third maze, again of the same pattern, had an olfactory trail in the true path.[1] The figures from the open maze are not compared directly with the others but with the score from the same maze when the restraining walls to the alleys were in place.[2]

We will not stop to comment upon the time of learning, as the chief differences are seen in the accuracy records. The olfactory maze heads the list in this respect both in initial, final and total accuracy within the limits of the experiment. The black-white maze follows second in initial accuracy and in total, because of the initial, but falls far behind in final accuracy.

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[1] In these tables the combined black-white figures are used, i.e., two groups of animals of five each where the true path was white and the *cul de sacs* black, and two groups of the same size where the conditions were reversed. The olfactory records are those where the trail was in the true path since this seemed most typical for our purpose. The combined olfactory scores make but little difference in the rating, they simply somewhat lowered the scores in the early trials and raised them in the final. For full details consult the previous papers.

[2] These mazes are all described in previous papers.

The reasons for this have been discussed in another paper. The time records are almost a direct reflection of the accuracy for, although the final scores distinctly favor the olfactory and black-white mazes, the advantage is but slight. The cutaneous open maze is also better than the normal in all of the accuracy counts, and also in final and total time. The small differences in time again are probably more or less an expression of the accuracy with a balance in favor of the open maze.

In these results, then, the olfactory maze leads, the open maze stands next in order while the black-white and the normal mazes approach each other very closely if all of the counts are considered. It will be seen that the big advantages which these sensory mazes offer are found in the early trials, in the setting up of the automatisms, and that the apparent total gain is due, for the most part, to the gain in the early trials. The established habit is practically the same no matter under what sensory conditions it is set up. The distracting effects of some of these sensory situations which affect the final scores are discussed in the previous papers.

Why some experimental workers should choose to neglect the error records is still a mystery to some of us. Certainly the real differences in the bits of learning given above are not shown by the time records. It is more essential in the normal life of a rat that it should be able to thread a beam or a cornice without falling, to jump from one projection to another without missing, to avoid being cornered and to strike its hole exactly when pursued by its enemies than to run a certain number of feet per second. The total distance which has been a measurement warmly advocated has advantages but it does not show really significant variations from the true path, although there is always, we will grant, a relation between the total number and amount of the errors and this total distance. If a rat always took the shortest way home, if it did not have its own peculiar way of getting out from its hole, perhaps this measure would express the facts more truly. The animal runs out into the maze a little way, runs back, runs out a little farther and then back, etc. This is a purely instinctive activity quite on a par with those natural movements in and about a rat's hole that insure it an open way home and such actions may not at all signify that the path as far as traversed is not familiar.

These runs then, which are included in the total distance, are not of the same nature as those other errors which take it entirely off the trail. They indicate its method of learning. They are interesting of course, and perhaps valuable for learning but they are not of the same class as the others. In all of this work, since our interest lay in the ability which the animals possessed to follow or to neglect a certain sensory stimulus, the errors consisted in leaving the true path. But it is not alone the number and kind of errors which excite our interest in this problem but also the distribution of the errors within the maze itself.

DISTRIBUTION OF ERRORS

The following tables show the distribution of errors in the different mazes. Whether a *cul de sac* will be entered or not depends upon the general direction in which it extends, its relation to the food box and whether it is so placed that the animal enters it headlong, etc., etc. For these reasons and for the sake of fairness we have chosen to compare the total scores of alleys 1, 2 and 3 with the corresponding error scores of 5, 6 and 7,[3] in the Hampton Court Maze. The normal maze record shows a greater score on every count for the first three than for the last three alleys. The combined black-white maze table reveals the same thing, as does also the cutaneous normal records. In the cutaneous maze, alleys 4 and 5 were very near to the food box, so near that in the open maze the animals sometimes succeeded in jumping across. For this reason these alleys were very attractive and the different results in the open maze may, perhaps, be explained in this way. It will be observed, however, that in the last three counts in the open maze also, when the automatism is beginning to be perfected, favor the final alleys. The olfactory maze, where the trail is in the true path, gives a record where the conditions are reversed, but when the trail is in the alleys the normal standard is again approached although the degree of difference between the first three and the last three alleys is less. The elimination of the final members of the series first is not only true of the groups as a whole but also of the individual animals. The records of 41 rats in the normal, black-white and cutaneous groups were gone over and only five rats found where the relation was reversed and two where they were the same. The number might have been easily doubled but it seemed useless to do so.

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[3] The *cul de sacs* are numbered in the order in which they occur in the maze.

TABLE III

Distribution of Errors, Normal Maze

	Alley 1	Alley 2	Alley 3	Alley 4	Alley 5	Alley 6	Alley 7	Alleys 1, 2, 3	Alleys 5, 6, 7
Total errors	81	112	146	82	68	121	25	309	214
Trial at which the error was not made three times in succession by any rat	18	21	26	5	13	16	11	31.6	13—
Average errors per rat after 10th trial	1.1	1.5	2.2	.7	1.2	1.	.6	1.6	.9
Average error per rat from 20th to 35th trial	.3	.9	1.	.3	.2	.1	.1	.7	.1

TABLE IV

Distribution of Errors, Black-white Maze

	Alley 1	Alley 2	Alley 3	Alley 4	Alley 5	Alley 6	Alley 7	Alleys 1, 2, 3	Alleys 5, 6, 7
Total errors	31	55	45	43	11	24	5	131	40
Trial at which the error was not made three times in succession by any rat	16	19	7.5	9	4.5	6	2.5	14.2	4.3
Average errors per rat after the 10th trial	.6	1.7	.8	.6	.1	.1	.1	1.	.11
Average errors per rat from 20th to 35th trial	.2	.6	.3	.5	.1	.05	.15	.36	.1

TABLE V

Distribution of Errors, Olfactory Maze, Trail in True Path

	Alley 1	Alley 2	Alley 3	Alley 4	Alley 5	Alley 6	Alley 7	Alleys 1, 2, 3	Alleys 5, 6, 7
Total errors	1	14	12	16	14	36	17	27	57
Trial at which the error was not made by any rat three times in succession	1	11	17	16	10	14	9	9+	11
Average errors per rat after the 10th trial	0	0	1.1	.9	0	.3	.3	.4—	.3—
Average errors per rat from 20th to 35th trial	0	0	.5	.1	0	0	.1	.16	.03

TABLE VI

Distribution of Errors, Olfactory Maze, Trail in the *Cul de sacs*

	Alley 1	Alley 2	Alley 3	Alley 4	Alley 5	Alley 6	Alley 7	Alleys 1, 2, 3	Alleys 5, 6, 7
Total errors	0	40	59	62	35	73	22	99	130
Trial at which the error was not made by any rat three times in succession	0	7	32	6	5	10	8	13	7+
Average errors per rat after the 10th trial	0	2.1	2.3	5.3	2.3	1.6	.3	1.46	1.4
Average error per rat from the 20th to the 35th trial	0	1	1.5	4.6	1.1	1.	.1	.83	.7

TABLE VII

Distribution of Errors, Cutaneous Maze—Sides Enclosed

	Alley 1	Alley 2	Alley 3	Alley 4	Alley 5	Alley 6	Alleys 1, 2, 3	Alleys 4, 5, 6
Total errors	53	25	52	60	19	1	130	80
Trial at which the error was not made three times in succession by any rat	26	17	18	23	8	2	20.3	11.
Average errors per rat after the 10th trial	2.6	.8	1.6	2.1	.5	.1	1.6	.9
Average errors per rat from the 20th to the 35th trial	.8	0	.6	.6	0	0	.5	.2

TABLE VIII

Distribution of Errors, Cutaneous, Open Maze

	Alley 1	Alley 2	Alley 3	Alley 4	Alley 5	Alley 6	Alleys 1, 2, 3	Alleys 4, 5, 6
Total errors	30	29	48	66	37	4	107	107
Trial at which the error was not made by any rat three times in succession	33	17	17	25	10	2	22.3	12.3
Average errors per rat after the 10th trial	3	2.5	2.3	3.1	2.6	.3	2.6	2.
Average errors per rat from the 20th to the 35th trial	1.6	1.3	1.3	.6	1.1	0	1.4	.5+

In a normal maze, when the *cul de sacs* are at all comparable, the number and persistence of the errors of the first part of the series may be explained by the laws of association, i.e., they are made first, made most frequently and therefore persist longer. On the other hand, it may be that the food box as the final and probably the strongest member of the motor series may become more directive and react back into and help to organize the later members of the series most closely connected with it in time much as other memory series—not motor—are known to be organized.

The black-white maze influenced the distribution of errors but as the rat's vision is notoriously poor the influence was chiefly seen in the smaller amount of difference between the records of the first and the last *cul de sacs* as the character of the distribution remained the same.

In the open maze the errors were more evenly distributed for the reasons given above. The animals in the olfactory maze were really learning to follow a trail and incidentally learning a motor series. The incidental errors increased toward the end of the series although the last two counts show a balance in favor of 5, 6 and 7. The experiment when the trail was in the *cul de sacs* gave a situation where the true path resembled that of the normal maze but the *cul de sacs* were made more attractive because of the odor and thus influenced the totals. The last three counts, however, are lower for 5, 6 and 7 in this experiment also.

Sensory clues in these mazes, not only favor accuracy but also affect the distribution of the errors among the members of the series of blind alleys. The influence is seen both in the degree of difference and in the character of the distribution. The distribution could be said to be less mechanical and not quite so predictable as in the normal maze although on the whole the relations were similar in all of the mazes.[4] The final members of the *cul de sacs* were entered less frequently and eliminated first.

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[4] Since making these tabulations and comparisons Miss Hubbert's paper has appeared, " Elimination of errors in the maze," *Jour. Animal Behav.*, Vol. 5, No. 1. Her results contradict those reported here, but this work must stand on its own merits. I cannot at this place enter into a discussion of Miss H's. paper. It scarcely seems fair, however, when there are so few *cul de sacs*, to eliminate the first and the last, where the chief differences are seen, from the comparison. There is always overlapping in the middle of any series. If the nature of the maze compelled this elimination then no general conclusions should be drawn.

Contributions from the Zoölogical Laboratory of the Museum of Comparative Zoölogy at Harvard College, No. 262

THE GRASPING ORGAN OF DENDROCOELUM LACTEUM

ELIZABETH S. P. REDFIELD

The grasping organ of Dendrocoelum lacteum is situated on the ventral surface of the anterior part of the head of this worm, between its two auricular appendages.

Ijima ('84) records this organ as used for locomotion against currents. Gamble ('96, pp. 35-48) describes this species as "affixing a sucker, placed on the under side of the head, to the substratum, and pulling the posterior end close to this. The sucker, discovered by Leydig, is even better developed in (Planaria) punctata, P. mrazekii, and P. cavatica, and is an efficient adhering-organ, which has probably been developed from a similar but simpler structure found in a considerable number of both fresh-water and marine Triclads."

How far these interpretations apply to Dendrocoelum lacteum, will appear in the following account. This paper is based on an experimental study of the function of the grasping organ in Dendrocoelum lacteum, and I wish to thank Dr. G. H. Parker, under whom the work has been done, for his kind assistance.

STRUCTURE OF THE GRASPING ORGAN

The grasping organ of Dendrocoelum lacteum, when studied under a low power of the microscope, appears as two symmetrically placed opaque thickenings, near the middle of the anterior margin of the head. In the resting condition, these structures project forward very slightly, forming a pair of rounded lobes, as shown in figure 1, *A*.

As the planarian moves about in ordinary locomotion, this organ is in continual activity, stretching out, contracting, now grasping an object, now rejecting it. When grasping at materials these lobes are stretched forward fully twice their resting length and press the object, one on each side (fig. 1, *B*). The form of

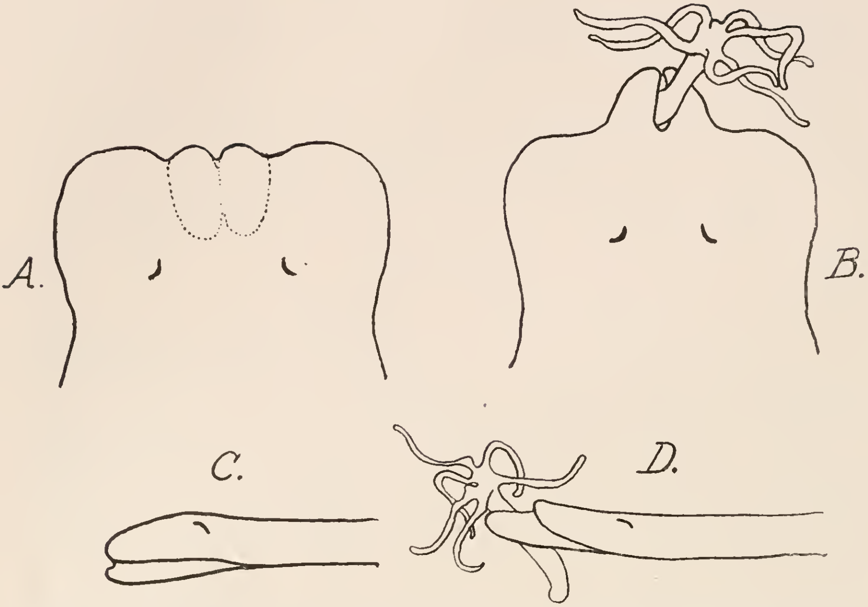

FIGURE 1. Head of Dendrocoelum; *A* and *B*, dorsal views; *A*, grasping organ contracted; *B*, grasping organ expanded; *C* and *D*, lateral views; *C*, grasping organ contracted; *D*, grasping organ expanded. Magnified 25 diam.

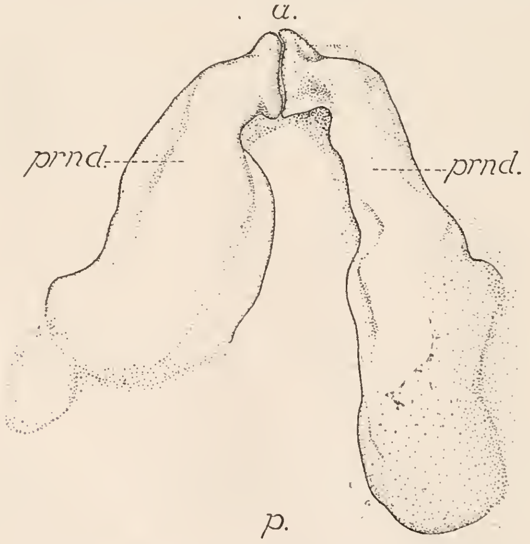

FIGURE 2. Ventral view of grasping organ expanded; drawn from a wax reconstruction; *a*, anterior; *p*, posterior; *prnd*, graspers. Magnified 50 diam.

the grasping organ as seen in surface view, is well represented in figure 2. It was with great difficulty that a planarian was killed with the grasping organ extended, and the preparation from which figure 2 was drawn was the only one in perhaps a hundred to show the organ favorably.

The organ consists of two ridges on the ventral side of the head, which converge abruptly and meet as two thickened, club-shaped lips at the anterior end. Posteriorly the ridges become broad and low, and merge into the ventral surfaces of the head.

Evidently this specimen was fixed in a condition of semi-extension, for a transverse section of another Dendrocoelum (fig. 3) shows the median groove to be greatly narrowed and the ridges on either side to be folded together.

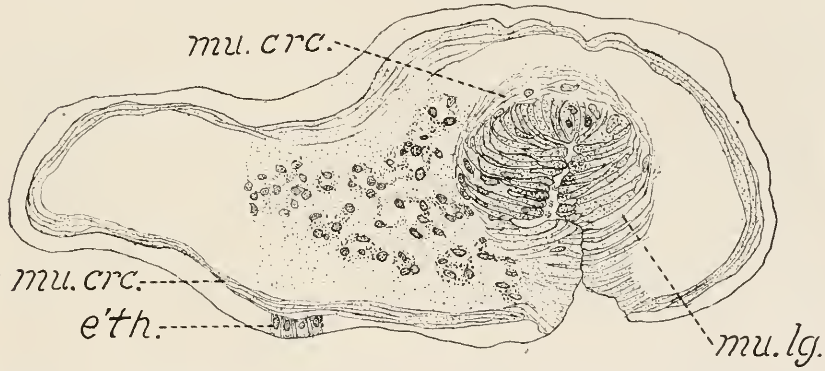

FIGURE 3. Transverse section of grasping organ showing musculature; *mu. crc.* circular muscles; *e'th.* epithelium; *mu. lg.* longitudinal muscles. Magnified 75 diam.

There are no rigid structures to be discovered in sections of the grasping organ. Such sections show an abundance of muscular tissue (fig. 3). It would appear probable that the grasping organ is extended through the action of these muscles upon the body fluids, thus producing a pressure which would stretch the anterior part of the ridges and perhaps give to the head an upward turn, thus bringing the ventral ridges into an anterior position. Figure 1, *C* and *D*, illustrate how this action may take place. The movement is too quick to allow of very accurate analysis, but it is evident from the way in which this organ is used and from the structure as seen in figure 1, that it is more appropriately designated a grasping organ, than a sucker.

FUNCTIONS OF THE GRASPING ORGAN

Certain classes of materials, such as substances suitable for food, call forth in Dendrocoelum a very striking reaction, which involves the grasping organ. If, for example, a pair of forceps which has been dipped in a twenty per cent cane-sugar solution is held in front of a Dendrocoelum, the animal will suddenly dart its head forward and seize the forceps with the grasping organ, and adhere to them so firmly that the creature can be shaken loose only with difficulty. The word "pounce" describes this reaction more vividly than any other, and the idea of a pouncing worm can surprise the reader no more than the reaction did the author.

A moving bait will call forth this reaction more readily than a motionless one. Living Hydra, and pieces of meat are seized by Dendrocoelum lacteum in this manner. These facts suggest that the grasping organ is used to capture and hold the prey of the planarian, until the pharynx can be affixed and feeding begin. To ascertain whether the grasping organ was primarily concerned with feeding, the following experiment was tried. A solution of brown sugar, which was visible in water, was applied by a capillary tube to various regions on the surface of the worm. On stimulating the lateral edges there was a local expansion of the body, a condition to be observed anywhere between the posterior end and any point almost as far anterior as the auricular appendages. In the neighborhood of these appendages, the application of the sugar solution caused the head to be turned towards the region of stimulation, the grasping organ to seize the end of the capillary tube, and feeding to begin.

Animals tested in this way with a capillary tube containing only water instead of brown-sugar solution, gave no reaction, and consequently the responses to the sugar solution could not be regarded as due to the mechanical disturbance produced by the slight current of water. From this experiment, it seems clear that the receptors which initiate the feeding reaction of the worm are located upon its head.

As a further test of this conclusion, the following experiments were made. Worms from which the brain, eyes, and auricular appendages had been removed by decapitation, were put each in a Syracuse watch-glass full of water and left standing twenty-four hours. A twenty per cent brown-sugar solution was then applied from a capillary tube to their lateral margins. In no case was there a response of the whole worm, as when feeding begins, though in each instance a local expansion of the side of the body occurred where the sugar solution had been applied. As a preliminary experiment, before removing their heads, all the worms were tested with a brown-sugar solution, and found to feed upon it in a normal manner. The heads of the decapitated worms were kept, each in a watch glass, and subjected to the same stimulus as that used on the trunk of the worm. They responded by coming up to the tube and remaining close to it, the grasping organ being agitated in the same manner as in the normal worm about to begin feeding. Occasionally a head would circle away from the tube but it always came back again.

These experiments confirm the previous one, in that they show that the receptors for the feeding reactions are located upon the head of the worm. One might object to drawing this conclusion on the grounds that animals whose heads had been removed might be expected, as a result of the operation, to fail to respond normally; but since the heads, which had been subjected to as great a shock as the trunks, if not a greater one, reacted normally, this objection can have no weight. In order to determine whether the grasping organ itself, or the auricular appendages were the receptors, the following experiment was tried.

The grasping organ, after having been stimulated to action, was removed by catching it with forceps and pulling it out. The results were surprisingly uniform; worms without the grasping organ did not attempt to feed. It would appear from this experiment that the receptors for the feeding reflex, are located probably on the sucker itself.

Mechanical and certain chemical stimuli (as a twenty per cent solution of sodium chloride) call forth a rapid forward movement of the animal, like that of some leeches, produced by (1) extending the body, (2) attaching the grasping organ to the substratum, (3) releasing the posterior end, contracting the body, (4) attaching the posterior end, and then again (5) carrying the body forward, thus repeating the whole operation. If the grasping organ is removed, the animal may still progress in a leech-like manner, attaching itself by the action of the general ventral surface of the anterior end combined with that of the margins of the body in the same region. If the head is cut off, the animal can also still progress in a leech-like manner. Consequently the grasping organ is not essential for this mode of locomotion. Animals in a dish of water held in a current, attach themselves to the substratum by the tail, but not by the grasping organ, thus showing that the grasping organ is not used to prevent the animal from being washed away.

In my own experiments, however, there is nothing to show that the grasping organ may not be used for locomotion; the point insisted upon is the importance of its relation to feeding.

SUMMARY

From the foregoing it is evident that the grasping organ of Dendrocoelum lacteum is primarily employed by the animal in feeding. This grasping organ is used to seize and hold material on which Dendrocoelum lacteum feeds. It is stimulated to activity by appropriate materials applied to the receptors located on the anterior part of the worm. The grasping organ may be used in certain forms of locomotion, but it is not essential to this operation.

POSTSCRIPT

Since the completion of this paper, a note by Julius Wilhelmi, Einige biologische Beobachtungen an Süsswassertricladen, has been published in the Zoologischer Anzeiger, Bd. 45, pp. 475-479, in which these grasping reactions are recorded.

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THE HABITS AND NATURAL HISTORY OF THE BACKSWIMMERS NOTONECTIDAE

CHRISTINE ESSENBERG

From the Zoological Laboratory of the University of California

The purpose of this article is to record observations made on the aquatic Hemiptera Notonectidae. These insects were chosen for the experimental work because they are found in great abundance and because very little is known about their behavior. The work was done in the Zoological Laboratory of the University of California under Professor S. J. Holmes, to whom I wish to express my gratitude for kind suggestions and criticisms. Thanks are also due to Professor C. E. Van Dyke for his help in determining the species.

The Notonectidae are commonly known as backswimmers from their habit of swimming on their backs. They are widely distributed, extending from the arctic to the tropical regions. Kirkaldy has recorded about twenty different species of Notonectas. According to J. R. de la Torre, Bueno, twelve of these species are peculiar to America. The bugs here described were collected in a small pond on the university grounds, the species identified being four in number, *Notonecta insulata* Kirby, *Notonecta undulata*, variety *Charon*, *Notonecta indica*, and an unidentified species. The last named is most abundantly represented. *Notonecta insulata* is the largest of the four species, ranging in size from five to five-tenths mm. in width. It is usually of a dark or bluish-black color. *Notonecta undulata* and *Notonecta indica* are smaller and more slender than *Notonecta insulata*. Notonectas show many striking adaptations to aquatic life: their backs are convex and boat-shaped, the ventral surface being flat. The hind legs are long, specially flattened and fringed, thus serving as oars. The two pairs of forelegs are sparsely covered with hairs and are provided with claws. The latter serve for the capture of food and for attachment to the surface film, from which they hang with their heads downward, the posterior part of the ventral surface being exposed to the air. When in this position the fore- and middle-legs are slightly bent so that the claws are at the surface. The insects often rest at the bottom, clinging to sticks or weeds.

It is also interesting to note that the hairs of the body are so arranged as to facilitate respiration. On the forelegs are two rows of hairs pointing in opposite directions, and partly covering the spiracles on the thorax. The ventral surface of the thorax has a double row of thick hairs on both sides of the margin, pointing posteriorly and meeting the hairs of the abdomen. There is also a double row of hairs on each lateral margin of the abdomen; the hairs of the outer rows increase in length as they approach the posterior end of the abdomen, where they end in one row of long tufts. The inner rows of marginal hairs cover the pleura and can be flapped back by the contraction of the abdomen. The carina or the ventral midpart of the abdomen is thickly covered with hairs which extend laterally on both sides and overlap the hairs from the lateral margin, which extend toward the middle line. Thus the rows of hairs form a waterproof covering over the gutters which lie one on each side of the carina and serve for the conveyance of air. In addition to the rows of long hairs described, the whole surface of the abdomen is covered with short hairs. On the dorsal surface, beneath the wings, there is a row of hairs between each segment, pointing posteriorly, while fine hairs cover the entire dorsal surface of the abdomen. The hairs are covered with an oily secretion which prevents their getting wet.

Three pairs of spiracles are found on the thorax, and a pair on each segment of the abdomen in the pleura. The air finds entrance to the spiracles from the posterior end of the body, where an opening is formed by the tufts of hairs as soon as the animal reaches the surface of the water. The hairs cling together and close the opening as soon as the animal is submerged. Sometimes the whole fringe is lifted like a lid from the pleura when the animal reaches the surface of the water, closing again as soon as it sinks. The bug comes to the surface to receive a fresh supply of oxygen and to emit carbon dioxide. The bug being surrounded by air, is lighter than the water so that it is compelled to keep the rowing legs in constant motion in order to keep beneath the surface. As soon as the leg-motion ceases, the air buoys the insect up until it meets the surface film. As soon as the animal is suspended by the surface film, it begins to move its forelegs. Hoppe suggests that by moving the legs the insect forces the air into the spiracles in the thorax. I am rather inclined to believe that it is trying to straighten out the hairs and to brush away the impurities, because the animal performs these movements every time some foreign substance is dropped on the thorax. If a small particle of asphalt be placed on the thorax, the animal moves the legs in an effort to free itself from it, and if it can reach the part, will also use the beak for the same purpose. It brushes the ventral and dorsal surfaces of the abdomen with the hind legs, and especially the tip and lateral margins.

Notonecta, if sealed in water, dies within from three to five hours. In this case it hangs to the surface film all the time with its body covered with gas bubbles. If a drop of oil is put on the ventral surface of its body, the creature dies immediately. If the backswimmer is sealed in the water from which the oxygen has been expelled by boiling, death results in from five to ten minutes. If the water is left exposed to the air, the insect clings to the surface film almost constantly, with the fringes lifted and the breathing pores exposed, while under ordinary circumstances it comes to the surface only once in every thirty or forty minutes. On a dry substratum, if exposed to sunshine, the insect dies within forty or fifty minutes; in a cool and shady place it can live much longer.

In several books on insects the statement is made that Notonectidae bury their eggs in the stems of plants. I have found that this is not the case in any one of the species studied. The ova are usually deposited and glued on sticks or on the stems of plants. Very often I have found the eggs deposited on the backs of other insects, such as dragonfly larvae, and on the walls of the aquarium. Soft sticks were placed in the aquarium. The eggs, however, were never found buried in the stems, but were always deposited in rows around the sticks. In the process of ovoposition, the female attaches herself to the lower surface of the stick and moves the posterior segments very vigorously until the egg is extruded and attached, then she moves away. Frequently embryos are found in a row with their heads pointing in the same direction. If the eggs are examined immediately after they have been deposited, the surrounding gluing substance is very plainly seen. The eggs are beautifully white, elongated, and cylindrical, with the attached side slightly flattened. The chorion is decorated with small depressions. The eggs of *Notonecta insulata* are about two mm. long. Those of smaller species are slightly smaller in size. After an incubation period of twenty days, the chorion splits at the anterior end and the nymph crawls out. In working out from the envelope the nymph first gets its head out, contracting and expanding the body and bending over it pulls itself out of the case, sometimes stopping to rest, then continuing the process again. Inside the coarse egg envelope and surrounding the nymph is a thin transparent membrane which occasionally breaks, allowing the animal to escape while the membrane remains attached to the outer envelope, but often the nymph is still surrounded by the membrane after leaving the egg-case and must work its way out from it. As soon as the nymph is free it goes to the bottom and rests a while and then begins to move about actively, searching for food. The nymphs are very beautiful, with large compound eyes which are conspicuously red. The body is transparent white. The long hairs are well developed on the lateral margins and over the pleura, which are shallower than in the adult; the hairs on the middle carina are short and irregular. After from seven to ten days the first moult takes place. Hoppe states that there are five complete series of moultings in *Notonecta glauca*, and J. R. de la Torre, Bueno,thinks that there are about five moultings in all the species. I have not been able to follow the complete series of moultings, because it is extremely difficult to raise nymphs in an aquarium owing to the fact that they attack one another even in the presence of an abundance of food. They are very sensitive to foul air and contaminated water. The nymphs differ from adults in their behavior in that they come more frequently to the surface, about once in every three or five minutes, and the fringes are usually flapped back, while in the adult the fringes are lifted only when the insect is in great need of oxygen.

The food of Notonectas consist of animal matter, chiefly, living or dead insects, but they do not hesitate to attack other animals. Thus Bueno has observed the nymphs of *Notonecta* *undulata* kill and suck the juices of young fish which had just emerged from the eggs. I have seen the nymphs and adults kill young *Diemyctylus torosus* as soon as they emerged from the gelatinous envelope. I have also observed a nymph dragging a Diemyctylus larva which was about three times its own size. The nymphs usually attack May-fly larvae and drag them about, although the latter are twice the size of the aggressor. The larger species usually attack the smaller ones and the young are eaten by the adults, being seemingly preferred to other kinds of food. *Notonecta* nymphs of equal age and size attack each other and suck each others juices during their attack until one or both are dead. They often attack dragonfly nymphs, which are many times their own size, usually approaching from beneath, grasping them with the forelegs and piercing the body with the proboscis. The nymph has no chance of escape from the insect, the latter continuing its hold no matter what position the victim may assume. The dragonfly nymph does not die from the first attack but only after a number of punctures. *Notonecta* is also destructive to young fish.

The Notonectidae are not easily affected by chemicals, as they may live in strong sodium chloride solution without showing any change in activity. They can live in five per cent alcohol for days. They become drowsy in twenty per cent alcohol, but if put in pure water they revive again. Several specimens were kept in strong copper sulphate solution for several weeks and did not suffer from it, while other animals dropped in the same liquid died within a few minutes. A Notonecta insulata was kept alive in Gilson's fixing fluid over two hours.

Notonectas are positively phototactic and can be led by the light in any direction. If an aquarium containing Notonectas is left in the sunshine, the bugs move quickly to the lighted end of the tank, and fly toward the light, producing a buzzing sound. If the aquarium is placed between two lights of different intensities, the Notonectas usually collect near the light of greater intensity, as is shown in the following table:

Two lights, one of one hundred candle power, the other of thirty-two candle power, were placed one at each end of the aquarium. Every five minutes the lights were reversed and the insects moving toward each light counted.

One hundred candle power	Thirty-two candle power
29	5
30	4
29	5
27	7
34	0
30	4
29	5
24	10
31	3
30	0
25	9
21	13
25	9
23	11
23	11
26	8
28	6
31	3
20	14

If Notonectas are put in a high jar and light admitted from below, the bugs are more numerous at the bottom. If the light shines from above, the bugs are more numerous on the surface. This proves that Notonectas have a strong positive phototaxis. However, their phototaxis may be modified by some such factors as temperature. Thus, an increase in temperature results in an increase in phototaxis, as shown in table following:

Number of Notonectas, thirty-four. Two lights, one sixteen candle power, the other one hundred candle power, were placed at either end of the aquarium and were turned on alternately every five minutes and the animals counted. The number moving toward the light is indicated by a plus sign, those moving away or at random, by a minus sign. Temperature increased gradually.

Weak light		Temperature	Strong light	
+	—		+	—
18	16	16°	30	4
15	23		24	10
28	6	18°	30	4
30	0		29	5
32	2	20°	33	1
32	2		34	0
33	1	22°	34	0
32	2		34	0
34	0	24°	34	0
34	0		34	0
34	0	26°	34	0

34	0		34	0
34	0	28°	34	0
34	0		34	0
34	0	30°	34	0
34	0		34	0
34	0	32°	34	0
34	0		34	0

At temperature of sixteen to eighteen degrees the insects are slower in their motions. At a temperature of twenty to twenty-two degrees they become more active, and with a further increase of temperature they are still more active, moving toward the light hurriedly and remaining crowded there. In a temperature of thirty-two degrees and above, the water bugs are slower in their movements, dying in a temperature of forty to forty-two degrees.

With increasing need for oxygen as is the case when Notonectas are placed in a high jar which is kept in diffused light and the temperature gradually increased, the bugs always come to the surface. But if the jar, containing the insects, is put in a dark room and a light is placed below the jar and then the temperature gradually increased as before, the bugs, attracted by the light, remain at the bottom, becoming more and more positively phototactic with the increase of the temperature, beating their heads against the bottom of the aquarium until they die in an effort to get nearer the light. Rarely one or more escape from the center of attraction and rise to the surface. If, however, two lights are placed, one above, the other below the aquarium, and the temperature gradually increased, the backswimmers rise to the surface and remain there even after the light above has been turned off, regardless of the fact that the light below is still shining. The last experiment indicates that although the creatures are strongly positively phototactic, after they have departed once from the source of light, the need of oxygen overcomes their phototaxis.

The question arises, Why do the insects rise to the surface with the increase of temperature ? As I have explained above, the animals carry air under the hair surrounding the body and in the spacious air tubes. Thus they are rendered lighter than the water and must keep their appendages in motion in order to keep beneath the surface. The increase in temperature brings with it an increase in the activity of the animal with a resultant greater need for oxygen for the metabolic changes which are taking place, hence the bugs rise to the surface in order to obtain the oxygen from the atmosphere. That the bugs remain at the bottom of the aquarium in spite of the high temperature, indicates that there are two opposite forces in action, viz., the need of oxygen and the positive phototaxis. The latter is increased with the increase of the temperature and is evidently so strong that the animals, after they have once been attracted by the light, can not depart, but struggle to get to the light at the bottom until they die, while, after they have once departed from the source of light the tendency of the bugs, with the increase in temperature, is to gather at the upper light. And even after the upper light is extinguished and the lower light is shining, the bugs still persist in remaining at the surface, because the demand for oxygen is greater than the positive phototaxis. The bugs are probably led by instinct to seek the surface when they become aware of the need of oxygen, and again they are only led by their strong positive phototaxis to go to the bottom of the aquarium. In normal conditions the Notonectas rise to the surface at more or less regular intervals. The same is true in diffused light when the temperature is increased, the bugs always rise to the surface and remain there. The buoyant force plays an important rôle here, but it may be of secondary importance only.

If the insects are sealed in ordinary water, they first swim about, but later they remain at the surface all the time. They may go down for a moment but return immediately to the surface, usually remaining there until they die.

If Notonectas are sealed in water from which the oxygen has been expelled by boiling, the air carried by the bugs is absorbed by the water, hence the insects drop to the bottom and do not rise, although they try hard to reach the surface. In this condition they die in from five to ten minutes. In this case buoyant force is of least importance and it is only this specific response which leads the animals to the surface in their quest for oxygen.

If Notonectas are placed in boiled water in an open dish they immediately come to the surface, remaining there until the amount of oxygen in the water has increased. When a light was placed below the aquarium containing boiled water, the Notonectas collected around it, the majority of them dying in a few minutes, while a few repeatedly returned to the surface for oxygen, sometimes picking up their dead companions on the way and carrying them to the surface. These experiments were tried with adult Notonectas and with nymphs with the same results.

Cold does not destroy the phototaxis of Notonectas, but when the insects become chilled, they move more slowly toward the light. If kept in ice-water for some time, the insects become so chilled that they drop to the bottom. If the water is shallow the insects come to the surface when the temperature is increased but if the water is from fifteen to thirty cm. in depth, while they show signs of life as soon as the temperature of the water is increased, they fail in their attempts to reach the surface. Sometimes they succeed in rising a few inches, swimming obliquely, falling back after each successive effort. It is an interesting fact that, while under ordinary circumstances the bugs must be in a constant motion in order to remain beneath the surface, here the reverse is true, the bugs working in the greatest effort to reach the surface and falling back each time. Evidently they have lost their air in some way or another. Thus the animals may lie until an increase in temperature arouses them to greater activity. Metabolism goes on, the remaining oxygen is used up, and when the animals attempt to rise there is no surrounding air to buoy them up, and death from lack of oxygen is the result. While in shallower water they have more access to free oxygen and can more easily reach the surface of the water. Hoppe believes that the cold water dissolves the carbon dioxide more readily and that, therefore, the animals, losing the surrounding gas, are rendered heavier and sink down. The fact of the bugs' trying to reach the surface, leads one to believe that the effort is an instinctive one when the animals are in need of oxygen.

If Notonectas are exposed to an arc light they first show a tendency to negative phototaxis for a moment, then they fly to the light and are burned. Young nymphs are positively phototactic from the very first day. I have experimented with light reactions on Notonecta insulata which were only three or four hours old. When exposed to light in a dark room, they crowded to the lighted side of the aquarium.

Notonectas are negatively geotropic and are usually found at the surface when at rest. This, however, may be due to the fact that in an aquarium without any weeds or sticks the surface film serves as the only medium of attachment. A stronger evidence in favor of negative geotropism is that the bugs, when placed on the wall, always crawl upward. Again, when the bugs are placed in a high jar which is kept in diffused light or in perfect darkness, they always rise to the top as soon as the jar is reversed. But here we have to deal with two factors, the need of oxygen in the closed jar, and the negative geotaxis.

Notonectas are positively rheotactic, always swimming against the current. They have positive thigmotaxis and usually are attached to some object when at rest. Sometimes they are attached to one another, three or four in number.

SUMMARY

Notonectas are widely distributed. They are very voracious and attack animals many times their own size. The insects are well protected by the thick layer of surrounding air, hence chemicals have very little influence on them. Notonectas have a strong positive phototaxis. The phototaxis increases with the increase of the temperature and with greater light intensity. When exposed to arc light or to bright sunlight, they immediately take to their wings, flying toward the light. At a temperature of zero centigrade they become chilled, lose their air, and drop to the bottom of the aquarium and die if the aquarium is deep. They are positively rheotactic, always swimming against the current. The young hatch within twenty days and are well adapted to their environment from their very first day of life. They resemble the adults in their behavior and instincts.

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SOME OBSERVATIONS ON THE INTELLIGENCE OF THE CHIMPANZEE

W. T. SHEPHERD
Waynesburg College

The observations reported in this paper were made on two chimpanzees; Peter, an ape on the vaudeville stage a few years ago, and Consul, also lately, and I believe still, on the stage. However, the writer is not certain that the latter ape exhibited on the stage as Consul and observed by the writer was the Consul extensively mentioned in the newspapers when brought from Europe a few years ago. The manager represented the latter vaudeville star to be the original Consul.

The observations made on Peter included those made at two performances by him on the stage and in one private examination of him. The observations on Consul included seeing him perform on the stage once and a private examination of him by the writer. In the cases of both Peter and Consul the observer questioned the keepers concerning the animal's performances, habits and training.

Of course, had it been practicable, observations should have been made much more extensively to give satisfactory results. But we believe that the observations reported warrant at least a partial explanation of the apparently superior intelligence exhibited by these and similar animals.

We shall first give a syllabus of the conclusions to which the observations appear to lead, and the indications to which the observed reactions seem to lead.

The writer believes that the apparently superior intelligence of the apes is principally accounted for by:

1. The superior motor-equipment of the animals
2. The training which all show animals receive.
3. The semi-erect carriage of apes.

We note also that:

4. There are indications of intelligent imitation in the mental make-up of the animals.

5. There are indications of a low form of reasoning, or of crude ideas in the apes.

6. There are indications of more human-like emotions than monkeys such as the Rhesus manifest, e.g., sympathy.

7. They show superior capacity for intelligent reactions to that of any of the lower orders of animals. Probably, largely on account of superior motor-equipment and their upright carriage.

8. With all allowances made, apes are superior in intelligence to all sub-humans and so are nearer to man than any of the other lower animals.

OBSERVATION OF PETER ON THE STAGE

Peter, dressed like a man, sat down to a table, put on a napkin and ate food with a knife and fork. After eating, he struck a match, lighted a candle, lighted a cigarette and smoked. He gave his keeper, McArdle, a light for the latter's cigarette from his own.

Upon command from the keeper, the ape danced on the stage fairly well, much like a man, a sort of jig-dance.

When roller-skates were put on his feet, he skated around the stage skilfully. He appeared to skate as well as a girl whom he chased around the stage.

The animal got upon a bicycle himself and rode it around the stage. He chased the girl around the stage while riding the wheel. While riding, he drank water from a cup handed him. Then he skilfully rode between a number of bottles and cut a sort of figure 8 while riding between the bottles. The ape picked up a bottle and drank out of it while riding.

The animal rode the bicycle up an inclined plane on the stage. I noticed that he always increased his speed just before coming to the inclined plane.

After performing these feats, Peter undressed and went to bed, very much like a man does.

PRIVATE EXAMINATION OF THE APE

Upon command from the keeper, Peter took up a hammer and a nail and drove the nail into the wall, quickly and without observable awkwardness.

IMITATION

As a test of imitation, I took out my watch and pressed on the stem, slowly, and opened the watch three times while Peter watched my actions with attention and apparently with interest. Then I reached it to him: he held it and pressed on the stem correctly several times, as if to open it. However, he did not press hard enough, and the watch did not open. He thereupon attempted to open it with his finger nails. The keeper stated to me that the ape had not received any training on that act.

APE WRITING

I held out a writing tablet and a pencil to Peter. He at once seized them and began scribbling, i.e., making irregular marks on the tablet. I made, in his sight, the letter T; a very plain T, with simply one vertical and one horizontal stroke of the pencil. The ape made a rather poor T, the first time shown. He also made a W when I showed him once. Peter seemed to like to use the pencil and tablet.

Upon being ordered by his keeper, the animal put a handkerchief around my neck and tied it quickly and correctly when told to do so. He also untied the knot quickly.

He came and slapped me on the lower limb when the keeper bade him, though apparently with some reluctance. The animal would lie down and sit up when ordered to do so.

APE LANGUAGE

When told to do so, Peter articulated the word "mama." The ape spoke the word something like a foreigner speaks it. I noted, however, that the wife of the keeper pressed her fingers against the ape's under lip when he spoke the word mentioned.

Now, let us attempt to analyze the factors in the apparently superior intelligence shown in the actions of the ape, just recited. In these acts, it seems we may see in the superior motor-equipment of the animal one of the principal factors. Peter's comparatively perfect hands enabled him to use the knife and fork in eating and to handle a cup in drinking. His man-like lower limbs, his hands and his upright figure enabled him to ride the bicycle, to pick up a bottle and drink while riding, etc. His superior motor-equipment was also, as it seems to the writer, a principal factor in such feats as driving a nail, tying a handkerchief in a knot and untying it, etc., Dogs and other animals, if they had the intelligence, lack the requisite motor-apparatus to do such acts.

Another principal factor in all these acts was, doubtless, training. We know that horses, dogs, and even pigs may be trained to do many feats.

In the writing by the ape, his man-like hands together with training, probably accounts for it, though imitation is possibly a factor here. What accounts for his seeming eagerness to mark on the paper might, however, be an interesting question. It might be interesting also to test how far the ape might be taught to carry his writing.

Peter's articulation of the word " mama " was very possibly quite mechanical and parrot-like, perhaps not understood by himself. However, it would be interesting to test how far such speaking by apes might be carried.

Peter's correct attempt to open the watch looks like intelligent imitation. However, though the keeper assured me that the ape had had no training in that act, we might doubt the statement. Then, perhaps, we could account for the reaction by the ape's hands, his training and the well known curiosity of all monkeys. If the veracity of the keeper can be relied upon, we have here, as it appears to the writer, a case of intelligent imitation.

In the matter of the ape increasing speed to ride up the inclined plane, if training does not account for it, we appear to see evidence of something very like ideation or reasoning of a low order. If in this instance ideas are present, they are perhaps what Hobhouse has named " practical ideas," i.e., crude and unanalyzed ideas. The writer is inclined to believe that the latter together with motor-equipment and training are the factors involved.

OBSERVATION OF CONSUL ON THE STAGE

Consul did most of the feats which Peter had done, such as putting on a napkin and eating at a table, getting upon a bicycle and riding around the stage, riding between nine bottles, riding up an inclined plane. Consul did these acts in a similar manner.

The latter ape also performed some other feats: He poured out his coffee, picked his teeth, cleaned his teeth with a brush, cleaned his tooth-brush. He rode a wheel with a lamp on his head, held by himself while riding; he bored with an augur, put the rounds in and fitted together a ladder, with some help. He took a tablet and pencil and wrote, or the keeper said he wrote; I do not know what he wrote. He took down the receiver of a telephone and listened, or appeared to listen. The ape used a typewriter, that is, he pressed on the keys, so far as the writer could judge, and can remember, perhaps without knowing what he wrote.

Consul threaded a needle, cut paper into strips with scissors. He took a key and locked and unlocked a padlock, and did other acts requiring similar intelligence.

These acts by Consul, like similar acts by Peter, are perhaps accounted for principally by the animals' motor-equipment, erect carriage, and training. Some of them, such as riding up the inclined plane and increasing his speed to go up, again raise the question of ideation or a lower form of reasoning in the animals' mental make-up.

IN PRIVATE EXAMINATION

The writer did not note in Consul the good nature and sympathy shown by Peter. The former ape showed the brute in him by a certain roughness of manner and by not obeying his keeper very readily, etc.

In this connection we might mention that Peter showed evidences of affection for his keeper by such acts as putting his arm around the latter in a very human-like manner and kissing him. When I questioned Peter's keeper as to sympathy, etc., in apes, to let me see for myself, the keeper, in the ape's sight, pretended to have hurt his hand, whereupon Peter went to him, put his arm around McArdle, and by his acts gave very evident signs of ape sympathy. Peter acted in a similar manner when I also pretended to have hurt my hand.

From the actions which have been enumerated of these two chimpanzees, we may, as the writer believes, venture to conclude that:

1. The apparently superior intelligence of the chimpanzee is accounted for principally by;

(a) Superior motor-equipment.

(b) Training.

(c) Their semi-erect and biped position.[1]

2. There are some indications of ideas of a crude and unanalyzed character or of a lower form of reasoning in their mental equipment.

3. That whatever are the factors involved in their reactions, apes such as the chimpanzee are the most intelligent sub-humans of which we have knowledge. As anatomically they are superior to the lower orders of animals, by the criterion of structure as indication of intelligence, they should be more intelligent than their humbler congeners.

We must, as already stated, admit that more extensive observations should have been made on the individuals we have considered. More individuals should also be observed and experimented upon before drawing final conclusions on some of the points at least involved.

However, the present writer cannot but believe that these and similar apes are the most intelligent of the sub-humans. We feel that anyone who has observed their actions, who bears in mind their anatomical superiority—their physical structure as compared to that of any lower forms on the one hand and to that of man on the other—who has noted their semi-human looks and actions in general, cannot but agree with this latter conclusion.

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[1] NOTE.—As a corollary from (a) and (c) doubtless, the superior intelligence of other lower species of monkeys is accounted for in part by their motor-equipment.

THE HABITS OF THE WATER-STRIDER GERRIS REMIGES

CHRISTINE ESSENBERG
From the Zoological Laboratory of the University of California

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INTRODUCTION

The purpose of this article is to report the results of observations made of the aquatic Hemipteran *Gerris remiges.* This work was carried on in the Zoological Laboratory of the University of California under the direction of Professor Samuel J. Holmes, to whom I am indebted for many valuable suggestions and criticisms. I also wish to express my gratitude to Professor C. E. Van Dyke for help given in determining the species.

The Gerridae, commonly known as water-striders, are of world-wide distribution and include many different species. The specimens were collected from small pools in Strawberry Canyon, near the University Campus, Berkeley. They are dark brown in color, the dorsal surface of the abdomen being red. The ventral surface is usually gray and is furnished with a plush-like coating which repels the water. The nymphs are much shorter with bodies closely resembling those of adults, but with the plush-like coating not so well developed. The water-striders pass the winter as adults, hibernating under logs, rocks, rubbish, and in other sheltered places. In the early spring they emerge, lay eggs along the edge of grasses growing under water but near the surface, fastening them with a waterproof glue. The eggs hatch within three weeks.

These insects move backward and forward with equal facility, though the usual direction of locomotion is forward; but if the animal is approached from the front it moves backward very swiftly. It can also float on its back as has been observed taking place in the aquarium during and after the cleaning process of the insect, when it lies on its back for a considerable time and is carried by the water, moving its legs or else keeping perfectly quiet. When disturbed while on the water the insects betake themselves quickly to the land or among the weeds, and hide by clinging to the lower surface of the leaves or by lying quietly on the ground.

For its food supply the water-strider depends upon such living or dead insects as it finds floating on the surface of the water. Sometimes it catches mosquitoes flying above the water. In the latter case it sits quietly upon some aquatic plant and, as soon as the mosquito approaches, makes a swift leap and catches the insect, or when a mosquito is discovered at some distance on the surface of the water, the water-strider moves very swiftly towards it until it reaches its victim, when it seizes it with its raptorial forefeet. The food is never taken from under the water. Several individuals were kept in an aquarium thickly populated with mosquito larvae, although the insects had not received any food for several days and were in a starving condition, they did not touch the mosquito larvae, but as soon as a mosquito emerged from the pupa case it was caught and eaten. *Gerris remiges* is very voracious and will eat any animal matter, not disdaining its own kind. It does not hesitate to attack animals many times its own size. In the aquarium, where there is less chance of escape, the young nymphs usually fall victims to the adults, and the stronger ones, as a rule, feed upon their weaker companions. In the laboratory the water-striders were fed mostly upon flies, they being most easily obtained, but they ate other animal matter, such as ants, bees, butterflies, moths, Jerusalem crickets, etc. *Gerris remiges* is not particular in its choice of food and its sense of taste is not well developed. Upon different occasions the insects were fed flies, some of which had been previously soaked in quinine and alcohol and some, in coal oil. They first approached the flies carefully, then left them, but soon returned and attacked them in spite of the taste or odor. Three days later these same insects were fed flies which had been soaked in ammonia for twenty-four hours. The following morning they were dead. The insect attacks fresh and decaying matter indiscriminately.

Gerris remiges can live on land as well as in water. It runs with a jerking motion, making from four to six jumps in succession and then making a short stop. Very often it turns a sommersault and continues running without interrupting its course until it reaches a place of safety. There it lies quietly for from fifteen to twenty-five minutes, then suddenly begins its race again. The insect can right itself when placed on its back by turning over longitudinally, resting its body on the head or abdomen. If a water-strider is held up by some of its legs, it tries to free itself by pushing the object holding it with the remaining free legs, at the same time pulling the legs which are being held.

The water-strider is accustomed to cleaning itself and some times is engaged in this occupation for hours. It rubs one leg with another, then it rubs its proboscis and the ventral and dorsal surface of the body interchangeably. Very often it rolls over in the water during the cleaning process. If some foreign substance, such as dust or asphalt is put on the dorsal surface of the body the animal dives and rubs itself in the greatest effort to get rid of the substance. If the insect is left in a weak light it remains quiet excepting that it rubs its legs; if brought into a strong light, it swims towards the source of light as nearly as possible, and rubs its legs.

Feigning death is a characteristic of this insect, which is especially well developed in some individuals. In accomplishing this feat it crosses its forelegs and becomes perfectly rigid. It may be rolled on the floor, picked up and held by one leg, dashed with water, exposed to considerable heat or to strong light without showing any signs of life. If left alone it lies quietly for ten or fifteen minutes, then gradually livens up and begins to run. If touched, it again feigns death, and thus it may continue for hours. The insect can be artificially made to assume this condition by putting it on its back and holding the hind legs, at the same time gently tapping it on its ventral surface, or by holding it down and pressing on the dorsal surface. The first sign of death feigning is usually the crossing of the forelegs, then the body becomes rigid and the legs are drawn up close to it so that the whole body assumes a compact shape. The water-strider has been made to assume this position thirteen times in succession. Later it did not so readily respond to the stimulus and the successive periods of death feigning gradually decreased in length, the first periods lasting for from twenty-six to twenty-five minutes and the last periods lasting for from six to five minutes, only. The larvae of these insects are especially prone to feign death. When taken from the water they sometimes feign death and do not recover for an hour or longer. Feigning death is evidently not a voluntary act on the part of the animal, this condition being brought about by some physiological change. Professor S. J. Holmes cites similar experiences with *Ranatra fusca* also with some birds from which he pulled feathers while the birds were in this condition without producing any response.

Gerris remiges is positively phototactic. If it takes to its wings once in a while it always flies toward the light, producing a buzzing sound as it flies. When placed in an aquarium it swims toward light. It is more phototactic in strong light and in high temperature, less so in a weaker light.

Gerris remiges is negatively geotropic. If an individual is left in an empty jar it always crawls upward. If placed on the wall it crawls upward, never downward, although it may jump or fly to the ground. The same is true when it retires to sleep on plants, attaching itself to the lower surface of the plants, with head pointing upward. Although very swift of motion when on water, the insect remains perfectly motionless on plants and makes no effort to escape when picked up. Blind individuals act in the same way as do the normal ones in regard to geotropism, i.e., they crawl upward or opposite to the source of gravity.

Water-striders are positively thigmotactic, piling up in three or four layers, sometimes becoming so tangled up in their long legs that it becomes difficult for those in the middle to disentangle from the others. They also crowd together when frightened and when hibernating in the winter.

The water-striders are positively rheotactic and always swim against the current. Experiments have been tried with individuals in which one or both eyes were destroyed. When water was rotated in a dish they swam against the current.

The sense of smell of the water-strider was tested in the following way: A small drop of coal oil was placed on the upper edge of the wall of the aquarium upon which the insects were trying to climb. When they had almost reached the oil, which was gradually moving downward, they stopped, moved their antennae, bent backward and plunged into the water. This experiment was tried with coal oil and ammonia water interchangeably, the insects commonly responding in the same way. A drop of coal oil was placed on the surface of the water in a corner of the aquarium. When the insects, in calmly gliding around occasionally came near the oil, they stopped, moved their antennae, then retreated. When frightened and in rapid motion they sometimes came directly into the field of the oil, when they would swim back excitedly and try to escape by jumping at the walls of the aquarium at the opposite end.

These insects seem to have a sense of hearing. When, for instance, a door is slammed, or some loud metallic sound is produced, the animals immediately respond by moving backward. When wingless flies are dropped into the water and are buzzing, the water-striders hurriedly move toward them, while a dead fly may float for a considerable length of time without being discovered. This experiment was tried with *Gerris remiges* whose eyes had been destroyed, with the same result, the blind insects moving from all directions toward the source of sound. However, they do not respond to all sound equally well. A tuning pipe was attached to one end of a wire, the other end of which was in the aquarium. The tuning pipe was then blown, but it had little or no effect on the water-striders, which continued their movements at random on the surface of the water.

SUMMARY

Gerris remiges are very common and are widely distributed. They are found almost everywhere on the surface of the water, beneath the rocks and in crevices. They are swift of motion. moving forward and backward with equal facility. They are very voracious and carniverous, although they never attack animals below the surface of the water, but only those on the surface or on plants. They show no particular choice in the selection of food, eating any dead or living animal matter. If nothing is obtainable, they can live without food for weeks or months. They are specially given to the cleaning habit and may be engaged in this process for hours in succession. They are prone to feign death and may be artificially stimulated to do so several times in succession. They are positively phototactic.

Gerris remiges is positively thigmotactic, hence, usually found in groups or piles beneath rubbish and rocks. The insects also crowd together on the surface of the water. They are positively rheotactic and negatively geotactic.

They seem to have some sense of smell. Their sense of hearing is not well developed, but they detect a sudden jar, such as the slamming of a door, or drumming on the edge of the aquarium, etc.

The sense of sight is keenly developed, the insects detecting a moving object or a shadow very quickly.

In considering the economic aspect, these insects may be useful because of their contribution to the reduction of the number of mosquitoes which lay their eggs on the surface of the water, also because of their destroying the emerging young mosquito.

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NOTES

MATERNAL INSTINCT IN A MONKEY

ROBERT M. YERKES

To my friend and fellow investigator, Doctor G. V. Hamilton, I owe the opportunity to make the observations of the behavior of monkeys which it is the purpose of this note to report. Gladly I avail myself of this chance to thank him publicly for his generosity in placing his animals and experimental equipment wholly at my disposal during the present year, and for his unfailing kindness and sympathy.

On February 27 one of the monkeys of our collection gave birth, in the cages at Montecito, to a male infant. The mother is a *Macacus cynomolgus rhesus* who has been described by Hamilton[1] as "Monkey 9, Gertie, *M. cynomolgus rhesus*. Age 3 years, 2 months. (She is now, May 1, 1915, 4 years and 6 months). Daughter of monkeys 3 and 10. First pregnancy began September, 1913." The result of this pregnancy was, I am informed, a still-birth.

The second pregnancy, which shall now especially concern us, resulted likewise in a still-birth. Parturition occurred Saturday night, and the writer first observed the behavior of the mother the following Monday morning. In the meantime the laboratory attendant had obtained the data upon which I base the above statements.

At the time of parturition Gertie was in a 6 by 6 by 12 foot out-door cage containing a small shelter box, with an exceptionally quiet and gentle male (not the father of the infant) who is designated in Hamilton's paper as Monkey 28, Scotty.

My notes record the following, exceptionally interesting and genetically important behavior. On March 1, when I approached her cage, Gertie was sitting on the floor with the infant held in one hand while she fingered its eyelids and eyes with the other.

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[1] Hamilton, G. V. A study of sexual tendencies in monkeys and baboons. *Jour. of Animal Behavior*, 1914, **5**, 298.

Scotty sat close beside her watching intently. When disturbed by me the mother carried her infant to a shelf at the top of the cage. Repeatedly attempts were made to remove the dead baby, but they were futile because Gertie either held it in her hands or sat close beside it ready to seize it at the slightest disturbance.

Especially noteworthy on this, the second day after the birth of the infant, are the male's, as well as the female's, keen interest in the body and their frequent examinations of the eyes, as if in attempts to open them. Often, also, the mother searched the body for fleas.

Observations were made from day to day, and each day opportunity was sought to remove the body without seriously frightening or exciting the female. No such opportunity came, and during the second week the corpse so far decomposed that, with constant handling and licking by the adults, it rapidly wore away. By the third week there remained only the shriveled skin covering a few fragments of bone, and the open skull from the cavity of which the brain had been removed. This the mother never lost sight of: even when eating she either held it in one hand or foot, or laid it beside her within easy reach.

Gradually this remnant became still further reduced until on March 31 there existed only a strip of dry skin about four inches long with a tail-like appendage of nearly the same length.

The male, Scotty, on this date was removed to another cage. Gertie made a great fuss, jumping about excitedly and uttering plaintive cries when she discovered that her mate was gone. Whenever I approached her cage she scurried into the shelter box and stayed there while I was near. This behavior I never before had observed. It continued for two days. On April 2, it was noted that she had lost her recently acquired shyness and she no longer made any attempts to avoid me. As usual, on this date, she was carrying the remnant about with her.

The following day, April 3, Gertie was lured from her cage to a large adjoining compartment for certain experimental observations. After she had been returned to her own cage the remnant was noticed on the floor of the large cage. I picked it up. Gertie evidently noticed my act, for although at a distance of at least ten feet from me, she made a sharp outcry and sprang to the side of the cage nearest me. I held the piece of skin (it looked more like a bit of rat skin than the remains of a monkey) out to her and she immediately seized it and rushed with it to the shelf at the top of the cage.

Two days later the remnant was missing, and careful search failed to discover it in the cage. It is probable that Gertie had carelessly left it lying on the floor whence it was washed out when the cages were cleaned. On this date Gertie seemed quieter than for weeks previously.

Thus it appears that during a period of five weeks the instinct to protect her offspring impelled this monkey to carry its gradually vanishing remains about with her and to watch over them so assiduously that it was utterly impossible to take them from her except by force.

After reading this note in manuscript, Doctor Hamilton informed me that Gertie had behaved toward her first still-birth as toward her second. And, further, that Grace, a baboon, also carried a still-birth about for weeks.

I am now heartily glad that my early efforts to remove the corpse were futile, for this record of the persistence of maternal behavior seems to me of very unusual interest to the genetic psychologist.

A REPLY TO PROFESSOR COLE

WALTER S. HUNTER

I cannot permit Professor L. W. Cole's recent article in the Mar.—Apr. number of this Journal, entitled "The Chicago experiments with raccoons" to stand unprotested. Abstracting from the deplorable tone of the publication, I should like to draw attention to one or two points only. (1) Professor Cole interprets my position as a desertion of the sensory-motor hypothesis in favor of some vague imageless thought construct. I tried strenuously in the monograph on Delayed Reactions to make clear that the ideational function ascribed to raccoons and to the child F was of a strictly sensory content. This content in any case need not be visual. It is not necessary that mental content copy the stimulus in order to represent it. In the Delayed Reaction experiments the content could not be visual because a visual *sensation* cannot be revived or reproduced. The content of the representative factor was very probably kinaesthetic (Delayed Reaction, p. 75) and was associated with the light. These kinaesthetic sensations could be revived and used as cues to differential responses. This is mentioned in many places in the monograph and is summarized finally in the classes of animal learning on page 79. I can see no grounds for so odd a misinterpretation of my attitude. (2) Professor Cole is aghast at the use of the term "steeple" for "staple" on page 18 of my monograph. This error was probably due to a slip in the proof reading. Had Professor Cole read a few lines further down the same page, he would have found the perfectly proper usage. (3) On page 167 of his article, by quoting a portion only of a sentence which in its turn was in a vital context, Professor Cole grossly misrepresents my statements concerning odor controls. It is to be noted that a very different criticism is involved to that offered elsewhere by Professor Watson. (4) The only confirmation that my work offers of Professor Cole's is, I still believe, the agreement indicated on page 20 of my monograph.

I see no need for further comments either upon the Delayed Reaction or upon the work by Gregg and McPheeters.

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LITERATURE FOR 1914 ON THE BEHAVIOR OF THE LOWER INVERTEBRATES

S. J. HOLMES
The University of California

Allee (1) has tested the relation of rheotaxis in Asellus to metabolism, measuring the latter by means of the duration of life of the animal in solutions of KCN. As animals having a high degree of metabolism die more quickly in KCN a means is afforded of testing the metabolism of different lots. Those which showed the most strongly positive rheotaxis were those in which, other things equal, the degree of metabolism is the greatest. Certain modifying factors must be taken into consideration and for an account of these reference must be made to the original paper.

In another paper Allee (2) points out that the rheotactic response is especially adaptive in stream isopods and is more pronounced in *Asellus communis* from streams than in the same species from ponds. The distribution of stream isopods is largely accounted for by their rheotactic and thigmotactic reactions.

Allee and Tashiro (3) have studied the relations between rheotaxis in Asellus and the rate of production of CO_2, and find that in a given individual the reaction is positive when the CO_2 production is rapid and indifferent when the CO_2 production is low. Resistance to KCN is in inverse proportion to the production of CO_2. There are great differences in the reactions of different individuals and "the rheotactic reaction is an expression, not of the absolute metabolic rate of the animal, but of the relative metabolic rate to which the isopod is acclimated for the time being."

In a paper in general ecology of Folliculina Andrews (4) records a number of observations on the behavior of this form. Ordinarily the species lives in tubes secreted by its body, but it often leaves these and swims through the water; it is positively phototactic and thigmotactic, and these traits lead it to settle down upon the younger parts of plants to which it is usually found attached. There is a strong tendency for individuals to aggregate in groups.

Baunacke (5) has considered various organs which might possibly occasion the orientation of Limax and other mollusks to gravity. He excludes light, tactual, and chemical stimuli and finds that orientation occurs in a medium of the same specific gravity as that of the animals. The statocyst alone is considered to be the organ directly concerned in orientation to gravity. Mollusks from which this organ has been removed are unable to orient themselves to gravity in the normal way.

The orientation of crustaceans to gravity forms the subject of an extended discussion by Buddenbrock (7) who recognizes three distinct factors which conspire to preserve the normal position of these animals. There is (1) the tendency to present the dorsal surface to the light (Lichtrückenreflex) which the author finds to be widespread among pelagic crustaceans. Then there is (2) the orienting function of the statocysts, and (3) a general reflex dependent upon no particular organ which leads the animal to keep the ventral surface below. In some crustaceans orientation to gravity may persist after the destruction of both statocysts and eyes.

According to Cowles (8) the starfish *Ecinaster spinosus* moves toward a white wall and away from a black one.

Dice (9) has analyzed the factors involved in the vertical migrations of *Daphnia pulex*. At 20° C. Daphnias are normally positive to weak light but indifferent to light of higher intensity. Increase of temperature makes them less positive, while decrease of temperature makes them more so. Light of high intensity makes Daphnias positively geotactic, while a decrease of light intensity has the reverse effect. These responses help to explain the diurnal movements of these forms. During the day Daphnias, at least in certain localities, are found more in deeper water while they commonly rise to the surface at night. This migration is due in part to the direct influence of light, but more to the effect of light upon geotaxis. Temperature also is a factor, as Daphnias tend to become positively geotactic in high temperatures and negatively geotactic in low. This has a marked influence in the seasonal migration of these animals. Other minor factors of migration are discussed, such as age and wave action. There is no diurnal rhythm independent of the direct action of orienting agencies.

Ewald (10) finds that Daphnia has two distinct modes of reaction to the light, the orienting response and a response to change in light intensity. By subjecting Daphnias to intermittent light Ewald found that the orienting response was not effected by the frequency of interruption, provided that the same amount of light was received in a given time. For the shock reaction, interrupted light affords a much more effective stimulus. Both types of response harmonize with the Bunsen-Roscoe law. Daphnias respond to different colors, and not only to light intensity.

Fasten (11), in an account of fertilization in a species of copepods, describes the copulatory activities of the male.

Galiano (12) has described, without indicating any general conclusions or discussing his results, a number of experiments on the chemotaxis of Paramecium. Reagents were used similar to those which Paramecium encounters under natural conditions, i.e., culture fluids, distilled water, and dilute alkaline solutions.

Herwerden (13) placed Daphnias in a horizontal glass vessel one end of which was closed by quartz. When ultra-violet light was passed through the quartz into the water the Daphnias became negative; when a piece of glass was interposed the reaction was discontinued. In specimens in which the eye was destroyed there was no negative reaction.

Hess (16) finds that the ambulacral feet of the starfish Astropecten retract under the influence of light. If only a small extent of the ventral surface of one ray is illuminated the feet struck by the light rays retract while the others are extended. The oral tentacles of Holothoria show a similar reaction. In both cases red light has little effect but blue and green readily evoke the response. Serpulas (14) react to change in light intensity like color-blind people, i.e., without regard to wave lengths of differently colored lights. Balanus reacts in similar way, ceasing its movements upon a diminution of the light.

H. Jordan (17) has studied in detail the function of the contractile fibers of the body wall of holothurians. Holothurians, actinians, annelid worms and many other forms are grouped in a division which Jordan calls hollow organ animals. In these forms the absence of internal or external skeleton is in a way compensated for by the presence of fluids within the animal which, when under pressure, afford a certain rigidity to the body. There is a detailed study of the general physiological properties of the muscles of holothurians as well as certain fibers of the skin which while different from other muscle fibers are shown to be contractile. These latter Jordan thinks have a special function of maintaining the tonus of the body, while all the more nearly typical muscles have their function limited to simple contractility. There is thus a separation of functions performed by striated muscles of higher forms similar to that which Von Uexküll describes for the two sets of fibers supplying the spines of the sea urchin.

Just (18) finds that at Woods Hole, Massachusetts, the swimming of *Platynereis megalops* occurs in July and August in the dark of the moon. The breeding activities may be studied in the laboratory although the males are rather delicate and as a rule live only a few days in receptacles of sea water. The male in swimming coils spirally about the body of the female and works forward until he gets into a position in which the female may seize his tail in her jaws. Just thinks that the sperms are swallowed by the female and fertilize the eggs internally, after which ovulation takes place by the rupture of the body wall. After copulation sperm may be found in the pharynx "whence they escape through lesions in the pharyngeal wall to the coelom."

Kafka (19) has given a valuable and welcome summary of work on the sensory reactions of the invertebrates.

Kanda (20) finds that the anterior end of *Paramecium caudatum* and *Spirorostum teres* is heavier than the posterior end and therefore the orientation to gravity shown by these forms cannot be a merely mechanical one. Differences in pressure of water on the two ends or sides of the body are so slight as to be negligible, and besides both these forms orient negatively to gravity in solutions of greater specific gravity than their body.

After excluding other theories, Kanda concludes that the statocyst theory of geotropism is the most tenable.

Kanda (21) has also studied the geotropism of Arenicola larvae, subjected them to various salts solutions isotonic with sea water and noted the influence of different media on the sense of the response. Calcium and magnesium ions tend to reverse the normally negative response, but their action tends to be neutralized by sodium. The metallic ions are considered to be the influential elements in reversing geotropism. The usual positive phototaxis of the larvae may be reversed by the addition of sodium chloride or potassium chloride, but the action of these salts may be antagonized by calcium chloride or magnesium chloride.

In a paper on the biology of the snail (Helix) Kühn (22) treats of hibernation, loss of weight in winter, and reactions to drought in summer. Helix can be made to come out of its closed shell when placed under moist conditions. It does not take dry food if its body does not contain a considerable amount of water.

The death feigning reflex of arthropods is described by Löhner (23), who not only reviews a considerable amount of literature on the subject but describes several experiments of his own on different species of diplopods. `The destruction of the brain or decapitation makes the reaction much more difficult to elicit, but these operations do not destroy it entirely. If the nerve cord is cut the part anterior to the cut can be made to perform this reflex. The reflex is not shown by an isolated part of the body behind the fifth segment.

The reactions of Bursaria to food and the processes of digestion in this species have been carefully studied by Lund (24). Bursaria may reject certain kinds of solid materials while it takes in others, depending on the action of the ciliary mechanism of the oral cavity. Large particles either do not enter the oral sinus or are rejected before reaching the latter, while small particles may be carried into the deepest part of the sinus and then carried out in a stream which passes backward on the ventral side of the body. The amount and rate of food taking depends on the condition of the animal. Bursaria appears to have a faculty analagous to the sense of taste, as it rejects food particles impregnated with various chemicals. There is a selective elimination of the contents of food vacuoles, as indigestible substances that have been taken in are soon gotten rid of.

Mast (25) has made an extended reply to a paper on Euglena by Bancroft, which was reviewed in this journal last year. The question at issue concerns the method of orientation, Mast upholding the previous contention of Jenning's and himself that it is brought about through a more or less modified form of the "motor reaction." It is impossible to give an adequate presentation of the arguments of Mast in a short space, and reference must be made to the original paper.

Metalnikov (26) finds that Paramecia that had injected Sudan powder so that they contained an average of 20 food vacuoles enclosing this substance, almost completely failed to take in Sudan on the following day, although they had been kept in the meantime in a fresh hay infusion. They took in other substances, such as carmin, sepia and egg albumen in abundance. If a mixture of nutritive and non-nutritive substances be given the Paramecium takes both at first, but later rejects the non-nutritive and continues to take nutritive material.

Orton (27) gives an account of the feeding mechanism and feeding reactions in brachiopods and certain polychaetes and discusses the evolution of similar food-taking mechanisms and their reactions in unrelated groups of animals.

Echinus miliaris were found by Orton (28) to aggregate into groups in the period of sexual maturity. Males and females were most frequently associated, but groups were not infrequently found containing but one sex.

In an extensive review of work on actinians Pax (29) has given a very useful resumé of investigations on the reactions and natural history of these animals.

The Smithsonian Institution has reprinted a paper by Pearse (30) on the habits of fiddler crabs, which was reviewed in an account of the literature for 1912 published in this journal last year.

Powers (31) has experimented on the reactions of four species of crayfish (Cambarus) to CO_2, HCl and acetic acid. In CO_2 the species die in the following order: virilis, propinquus, diogenes, immunis. All four species react more strongly to HCl than to acetic, and more strongly to acetic acid than to CO_2.

There seems to be a correlation between the specific reactions of the species and their habitats. In all the species the behavior is repeatedly modified owing, in the opinion of the author, to "increased sensitiveness on the part of the animals."

Pütter (32) has given a resumé of experimental work on the irritability of protozoans.

In the course of a detailed study of the structure of the infusorian Diplodinium, Sharp (33) records several observations on locomotor activity and the action of the membranellae.

Torrey and Hays (34) find that *Porcellio scaber* is negative in its reaction to light and may be driven about in any direction by light coming from behind. The first reaction of the animal when light is suddenly flashed upon it is to turn directly away. Orientation, the authors conclude, is direct although the method may be obscured by various random movements.

In the course of an account of the influence of various chemicals on *Colpidium colpoda*, Weyland (35) describes results of experiments on the chemotaxis of the species in relation to a considerable variety of compounds.

Zagorowsky (36) finds that Paramecia are positively thermotactic up to 32° C. but at 33° C. and above they become negative. The rate of swimming increases with rise of temperature up to 49° C. after which it rapidly falls and ceases at 55° C.

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LITERATURE FOR 1914 ON THE BEHAVIOR OF SPIDERS AND INSECTS OTHER THAN ANTS

C. H. TURNER
Sumner High School, St. Louis, Mo.

TROPISMS

1. *Chemotropism.*—By attaching shallow pans, containing kerosene, to the branches of trees, Severin and Severin (77) were able to catch large numbers of the Mediterranean fruit fly (*Ceratitis capitata*). Out of 5,490 flies trapped in eighteen days only thirty were females. As these authors say, "It is certainly peculiar that the Mediterranean fruit fly plunges into kerosene to its own destruction." By using pans of four different colors (white, black, blue and orange) they demonstrated that the number of flies secured was not determined by the color of the pan. They think it highly probable that the sense of smell plays an important rôle in attracting the flies, and admit that it might be a positive chemotaxis due to one or more hydrocarbons or to the impurities of the petroleum oils. "Again, the hydrocarbons of the oil may act as an anesthetic, and stupify the insects whenever they remain within its influence." Neither of these suppositions, however, accounts for the preponderance of males. For a period of eight months these flies were trapped daily. During that time only three victims out of every thousand were females. Admitting that the proof is not conclusive, these investigators believe that the kerosene emits a scent similar to "some sexual odor of the female which in natural conditions serves to guide the male to her." This harmonizes with Howlett's interpretation* of the reaction of *Dacus zonatus* to the oil of citronella.

In another paper (78) these two investigators have discussed the relative attractiveness of vegetable and petroleum oils for the Mediterranean fruit fly.

2. *Hydrotropism.*—In two different species of water beetles,

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* Howlett, F. H. The Effects of Oil of Citronella on Two Species of Dacus. *Trans. Ent. Soc.*, London, 1912, pt. II, pp. 412-418.

that inhabit a pond of about 300 square feet, Weiss (98) observed interesting cases of what he calls positive hydrotropism. When the wingless beetles *Gerris marginatus* were removed one to nine yards from the pond they immediately returned to it. When removed ten yards from the water they had some trouble in getting started in the right direction; but finally reached the pond. Thirty yards from the water they seemed to be lost. The case of *Dineutes assimilis*, a winged beetle, is even more interesting. When removed nine or ten feet from the water, it tried to walk to the pond, then arose and flew directly to it. When removed to a distance of seventy-five feet, it walked about in all directions, then arose, on its wings, to a height of twenty feet and flew directly to the water. When removed half a mile from the pond, it soared in a widening sub-spiral to a height of seventy-five feet and flew off in the direction of the water. He does not know whether they reached the water or not.

3. *Phototropism.*—Beutel-Reepen (6) does not think swarming honey bees are positively heliotropic.

OLIGOTROPISM

Robertson (75) does not accept the opinion that " Therefore the entomophilous flora of a region, as a whole, is not better pollinated because a part of the bees are oligotropic than it would be if they were all polytropic." He writes: " My view is that the bee fauna is all that the flora will support, that there is a constant competition between bees, and that natural selection favors those which are the most diversified, i.e., the least competitive in food habits." He believes that short flight is a result of oligotropy. To show the reasonableness of his contention he insists that if a bee limits itself to a given species of flowers it gains the immediate advantage of being able to anticipate others in their visits to the chosen plant. By locating near the flowers, it may augment this advantage, and, by concentrating its attention on that flower, learn to manipulate its pollen to greater advantage and even develop special structures which will increase this advantage. In support of this last statement, he cites the following examples:—(1) Bees that collect large pollen have loosely plumose scopae, while others which collect from the compositae have densely plumose scopae. (2)

Oenothera, the evening primrose, has its pollen grains connected by threads. *Anthedon compta*, an oligotrope of this plant, unlike its nearest relatives, has scopae of long single bristles. The bee goes to the stem and turns head downward, so as to work the cob-webby pollen into the scopae. (3) The anthers of *Verbena* are included in a slender tube and above them is a circle of hairs which impresses one with the thought that they were intended to prevent the extraction of the pollen. *Verbenapsis verbenae*, the oligotrope of this flower, has her front tarsi provided with curled bristles. The bee thrusts both front feet into the corolla and drags out the pollen with her front tarsi.

According to Robertson, there are 223 indigenous nest-making bees. One species flying the entire season, and fitted about as *Apis*, might collect nearly as much pollen and support nearly as many individuals. It would be to its advantage to be as polytropic as possible. " The ecological specialization exhibited by *Anthedon*, *verbenapsis* and other oligotropes is a fairly certain indication of the pressure of competition." The long-tongued pygidial bees were developed as competitors of the bumble bees, the first on the ground and the most polytropic of all bees. This explains their short and rapid flight and their oligotropic habits. Likewise the *Andrenidae*, the *Panurgidae* and related groups, which are often oligotropic, were probably preceded by the *Halictidae*, the most polytropic of all short-tongued bees. There are forty species of *Halictidae* flying throughout the season. There are ninety-four other short-tongued bees occupying the same region. It would be a hard matter for all to fly throughout the season and compete with the *Halictidae*. They have short times of flight, so distributed that not more than fifty-two are flying in any month and these only in the spring, when the *Halictidae* are the least abundant. All these bees are least abundant when the *Halictidae* are most abundant. " The early maximum flight, the non-competitive phenological distribution, and the frequent oligotropic habits indicate that these bees have managed to hold their own only by dividing up the remaining field and occupying the most favorable corners left by their polytropic competitors."

Lovell's views upon the origin of oligotropism are diametrically opposed to those of Robertson. In his recent reply (52) he makes the following criticisms:—

1. There is no evidence to support Robertson's contention that *Epiolus* is a parasitic genus.

2. To Robertson's assertion that a strenuous struggle for food is the determining factor in the evolution of the habits of bees, he replies that the size of the bee fauna is limited by other factors than the food supply. He insists that the commonness of an insect species does not depend alone on the quantity of available food, and gives the following specific proofs that only part of the available food is gathered by bees:—(a) In Riverside County, California, the orange bloom secretes nectar so freely that it drops upon the teams and clothing of the pruners in such copious amounts that, at the close of the day, it is necessary to wash the horses and the harness. (b) Hundreds of acres of the sandy coastal plain of Georgia are covered with bushes of the common gallberry (*Ibex glabra*). It is in bloom for a month and secretes nectar constantly. According to J. J. Wilder these flowers are seldom visited by bees.

3 To Robertson's hypothesis that, in the remote past, oligotropism arose through the competition of the long-tongued pygidial bees with the *Bombidae*, and of the *Andrenidae* with the *Halictidae*, Lovell replies: "This highly imaginative supposition cannot be supported by historical data, and would appear to be neither probable nor necessary." The polytropism of *Halictus* is due to its peculiar economy. The impregnated females hibernate and appear the following spring. The new generation flies during the latter part of the season. "This economy has no special advantage, for *Halictus* is greatly surpassed by *Andrena* in both species and individuals; while *Sphecodes*, which has essentially the same economy as *Halictus*, is represented by comparatively few species and individuals.

It is an advantage for a social bee to maintain its organization throughout the season; but for a solitary insect it is desirable that it mate and deposit its eggs as soon as possible. The longer the female flies before this happens the greater the probability that she will be destroyed by some one of many causes. * * * Since many polytropic bees have either a short term of flight, or one which does not exceed a hundred days, it is clear that a shorter term of flight is not necessarily correlated with oligotropism."

4. If severe competition did exist among solitary bees the

oligotropic habit would not be desirable. It is not an advantage for a bee to confine its food to one kind of plant unless it is always certain to obtain the supply it needs. By overstocking a locality oligotropic bees would disappear or become polytropic.

5. The genus *Perdita* contains about 150 species, practically all of which are oligotropic. An examination of the habits and characteristics of the genus should throw some light upon the origin of oligotropism. The facts are these: (a) the species are mostly small; (b) they do not take long flights; (c) a part of the species are vernal, but the majority fly in late summer and autumn; (d) many visit the *Compositae*; (e) oligotropism is as pronounced where there is only one or a few species as where there are many; (f) many flowers are visited by more than one species of *Perdita*; (g) the length of the tongues of bees limit them to certain flowers, " thus it is the tube-length of the flower, not competition, which is the factor limiting the visits of many species of Perdita; (h) female inquiline bees do not gather pollen and nectar for brood-raising and require only nectar for themselves; nevertheless, many such bees, with short terms of flight, visit only the *Compositae*.

Lovell concludes: "According to the theory proposed by the writer certain bees have become oligotropic because of the direct advantage gained, combined with the fact that their flight was synchronous, or nearly so, with the period of inflorescence of the plant to which they restricted their visits. This theory offers an explanation of the rise of oligotropism by the observation of existing conditions. There may be, and often are, accessory factors, but they are of secondary importance. * * * Robertson concedes all that is required when he says, 'The average flight is shorter and there are more of them with a short flight.' "

AUDITORY SENSATIONS

Hitherto the contributions to the experimental study of the sense of hearing of butterflies and moths have been fragmentary. As far as the moths are concerned, Turner and Schwarz (89) and Turner (86) have attempted to remedy this defect. In their joint paper (89) these investigators report the results of laboratory experiments with *Catocala unijuga* and field experiments with *C. flebelis*, *C. habilis*, *C. neogama*, *C. piatrix*, *C.*

retecta, var. *luctuosa*, *C. robinsona*, *C. vidua*, *C. amica*, *C. epione*, *C. neogama*, *C. ilia*, and *C. innubens*. The human voice, the Galton whistle and organ pipes were used to produce sounds. Except for special reasons, these instruments were always sounded where they could not be seen by the insect. To test the ability of the moths to respond to sounds to which they were usually passive, the following method was employed. Simultaneously with the sounding of the note the moth was gently touched. This was repeated one or more times and then the pitch was sounded without the tactile sensation. These authors reached the following conclusions:—" 1. Our field experiments demonstrate that several different species of *Catocala* moths respond to certain high pitched notes of the Galton whistle; but that they do not usually respond to notes of low pitch, such as the rumbling of trains, etc. 2. Most specimens responded to a high note by flying to a nearby tree; but some, and this was especially true of *C. retecta*, responded by making quivering movements with its wings. 3. The degree of responsiveness was not the same for all species. Among the least responsive were *C. vidua*, *C. neogama;* and at the other extreme were *C. flebelis*, *C. habilis* and *C. Robinsoni*. 4. We do not consider the failure of these moths to respond to certain sounds of a low pitch a proof that they do not hear such sounds; indeed, we are inclined to believe that these creatures respond only to such sounds as have a life significance. Three things render this last supposition probable: (1) the fact that *unijuga*, which at first did not respond to whistling, did so readily after once a blast of air had been allowed to strike her body simultaneously with the sounding of the whistle; (2) that most of the natural enemies of these moths produce high pitched sounds and trains and brass bands and other producers of low pitched sounds do not directly affect the survival of these moths; and (3) by carefully conducted field experiments we were able to induce three specimens of *C. neogama* to respond to sounds to which the species does not usually react."

Turner (86) reports the results of laboratory experiments with 79 specimens of *Samia cecropia* Linn., 104 of *Philosamia cynthia* Drury, 41 of *Callosamia promethea* Drury, and 81 of *Telea polyphemus* Cramer. These experiments were conducted in a building so constructed that it was impossible for the vibrations of the sounding body to reach the specimens by any medium other than the air. For producing the tones, the following instruments were used: an adjustable organ pipe, an adjustable pitch-pipe, and an Edelmann's Galton whistle. The moths were confined beneath wire dish covers. Preliminary experiments demonstrated that each of the instruments could be held a short distance from the moths without causing a response, hence it was unnecessary to hide the instruments; precautions were taken, however, to prevent drafts caused by the instruments from impinging on the moths. To all ordinary tones *Telea polyphemus* is non-responsive. To see if this was due to deafness or merely to a refusal to respond to the stimulus, the following method was employed. "The organ-pipe was sounded five times in rapid succession. Immediately thereafter the insect was roughly handled for a few minutes. It was tossed about, gently squeezed and thrown upon its back. This was repeated over and over again, sometimes in one order and sometimes in another. After the moth had quieted down the whistle was sounded again five times in rapid succession. At each sound of the pipe the moth would wave its wings. The author has tabulated the effects of age, temperature and sex upon the responses. The paper concludes as follows:—"1. It seems certain that all four species of the giant silk-worm moths investigated can hear. Three of the species respond to a large range of sounds. The third, *Telea polyphemus*, normally does not respond to sounds, unless remaining as immobile as possible be considered a response. By experimentally causing the moth to associate some disagreeable experience with certain sounds, it can be induced to respond to these sounds. 2. There is much evidence that the response of moths to stimuli is an expression of emotion. The fact that an insect does not respond to a sound is no sign that it does not hear it. The response depends upon whether or no the sound has a life significance."

OLFACTORY SENSATIONS

See Severin and Severin under chemotropism.

Beutel-Reepen (6) discusses the olfactory sense of the honey bee and thinks Forel's flasks are olfactory organs.

In a series of papers McIndoo (56, 57, 58) has made a substantial contribution to our knowledge of the olfactory sense and of the olfactory organs of insects. He experimented with honey bees, hornets, ants and spiders. The following odors were used: oil of peppermint, oil of thyme, oil of wintergreen, bee food, pollen from old combs, parts of plants, flowers of the honeysuckle, leaves and stems of pennyroyal, spearmint, scarlet sage and bee stings. The odoriferous substances were isolated in stoppered vials. These vials were opened near, and usually below, the insects. Under normal conditions all of these creatures responded to the odors; usually by moving away from them. Evidently all of these forms can smell.

McIndoo thinks he has settled the debated question as to the location of the olfactory organs of insects. The belief that the antennae are the olfactory organs of insects is so widely spread that few except specialists know that the location of these organs is a debatable question. The following epitome of McIndoo's extensive bibliography will give an idea of the diversity of opinion on this subject. The seat of the olfactory organ is supposed to be in the spiracles by Sulzer (1761), Dumeril (1797), DuBois (1890), Hermbstadt (1811), Baster (1798), Lehmann (1799), Cuvier (1805), Straus-Durckheim (1828), Lacordaire (1838), Brulle (1840); located near the spiracles by Joseph (1877); in the glands of the head and body by Ramdohr (1811); in the oesophagus by Treviranus (1816); in the folded skin of the forehead by Rosenthal (1811); in the rhinarium by Kirby and Spence (1826); near the eye by Paasch (1873); in the mouth cavity by Huber (1814); in the epipharynx by Wolff (1875); in the palpi by Lyonnet (1745), Bonnsdorf (1792), Knoch (1798), Marcel de Serres (1811), Newport (1838), Driesch (1839), Perris (1850), Cornalia (1856), Weismann (1889); in antennae (belief based on structure) by Reaumur (1734), Lesser (1745), Sulzer (1776), Fabricius (1778), Bonnet (1781), Olivier (1789), Latreille (1804), Samonelle (1819), De Blainville (1822), Robineau Desvoidy (1828), Carus (1838), Percheron (1841), Goureau (1841), Pierret (1841), Robineau-Desvois (1842), Slater (1848), Dufour (1850), Claparede (1858), Donhoff (1816) Noll (1869), Wonfor (1879), Henneguy (1904); in antennae (belief founded on experiments) by Duges (1838), Lefebvre (1838), Küster (1844), Perris (1850), Cornalia (1856), Gardnier (1860) Balbini (1866), Forel (1874, 1885, 1908), Trouvelot (1877), Layard (1878), Slater (1878), Chatin (1880), Lubbock (1882), Plateau (1886), Graber (1887), Dubois (1895), Fielde (1901, 1903, 1905), Piéron (1906), Wheeler (1910), Barrows (1907), Kellogg (1907), Sherman (1909); in various structures on the antennae by Erichson (1847), Burmeister (1848), Vogt (1851), Wonfor (1874), Bergmann and Leucjhart (1852), Leydig (1860, 1886), Lowne (1870), Claus (1872), Mayer (1878, 1879), Reichenbach (1879), Hauser (1888), Kraepelin (1883), Schiemenz (1883), Sazepin (1884), vom Rath (1887, 1888), Ruland (1888), Nagel (1892, 1894, 1909), Dahlgren and Kepner (1908), Börner (1902), Schenk (1903), Rohler (1905), Cottreau (1905); Berlese (1906) in the caudal stylets by Paxkard (1870); on the base of the wings and on the legs by Hicks (1857, 1859, 1860), Bolled, Lee (1885), Hauser, Janet (1904, 1907).

To settle experimentally the question McIndoo amputated the antennae of certain bees, wasps and ants and covered the antennae of others with shellac or celloidin. Such mutilated bees were abnormal in their behavior; sometimes they would respond to odors and sometimes they would not. Bees with maxillae and labial palps removed responded to odors the same as normal bees. Bees with the proboscis removed, bees with the mandibles amputated and bees with the buccal cavity plugged with paste responded to odors. When the bases of the wings were glued and when the legs were covered with vaseline and beeswax the insects were much slower than usual in responding to scents. These experiments caused McIndoo to agree with Hicks that certain peculiar pores found on the base of each wing and on the legs are the olfactory organs. In his latest paper, McIndoo (56) makes the following criticisms of the researches of most of his predecessors:—(1) Most investigators study the behavior in captivity for only a short time and others did not investigate the behavior of the unmutilated individuals. (2) When the antennae are injured or removed the insect is no longer normal. (3) In the honey bee the pore plates can scarcely be considered olfactory, for the male has eight times as many as the female, but responds to odors less frequently. (4) The pegs may be eliminated because they do not occur in the drones. (5) Pore-plates are not the olfactory apparatus of insects, for they are entirely absent in the *Lepidoptera*. (6) Spiders smell; yet they have neither antennae nor any organ that corresponds to them. He closes with the following sentence: " In conclusion, it seems that the organs called olfactory pores by the author are the true olfactory apparatus in the *Hymenoptera* and possibly in all insects and that the antennae play no part in receiving the stimuli."

VISUAL SENSATIONS

See Severin and Severin under chemotropism.

Beutel-Reepen (6) thinks colors and odors attract bees to flowers.

Lovell (53) cites several examples of black animals and of people clothed in black being attacked by bees; while white animals, in the same situations, were unharmed, thus supporting the apiarists' belief that a beekeeper receives more stings when clothed in black than when dressed in white. In one experiment he wore a black veil and a white suit, with a black band on one of the sleeves. When the bees were disturbed they attacked the black veil and the black band, but not the white clothing. He repeated the experiment using a white veil and a black suit, with a white band on one sleeve. This time the bees attacked the black suit, but neither the veil nor the white band.

In another contribution (51), he makes the following objections to Plateau's statement that the odor of nectar is necessary to attract bees to flowers:—(1) "The cornflower and several gentians have odorless, conspicuous, nectiferous blossoms, which are visited by numerous insects. (2) The nectarless and odorless wind-pollinated flowers of the elm are visited by countless numbers of pollen-seeking honey bees. (3) The highly scented and conspicuous flowers of the sweet-pea and of certain varieties of *Pelargonium* are not visited by insects. Plateau claims that the odorless, but conspicuous, blossoms of the following plants are not visited by bees: *Clematis Jackmanni*, *Pelargonium zonale*, Willd., *Lilium candidum* L., *Pisum sativum*, *Passiflora adenophylla* Mostera, *Oenothera speciosa* Nuttall, *Linum candidum*. Discovering that the placing of the oil of anisette on these flowers would cause insects to visit them induced Plateau to conclude that it was the odor that attracted them. Lovell found that the placing of odorless sugar water on these flowers causes them to be visited by insects. According to Lovell, a certain man had two apiaries situated two miles apart. In the fields of one frequent rains had produced an abundance of clover with long corollas; in the fields of the other a drought had caused the clover to have short corollas. In the second clover-field the bees were so abundant that they stung the men who attempted to mow the clover. In the first field there were practically no bees. Evidently, the presence of the bees in the former field was caused by neither the color nor by the odor, but by the accessibility of the nectar. Lovell's paper closes with the following conclusions:—" Entomophilous flowers are usually characterized by the possession of either bright coloration, or odor, or both, although apparently to some extent the two qualities are mutually exclusive. Both allurements are useful in attracting the attention of insects; but the absence of either conspicuousness, or odor, or both, will not necessarily cause a flower to be neglected if it contains an ample supply of nectar or pollen. But under similar conditions, small, green, odorless flowers, even if rich in nectar, will not be discovered as quickly as nectiferous flowers, which are conspicuous or agreeably scented. On the other hand, the possession of both color and odor will not ensure frequent visits in the absence of available food materials. The experiments afford no evidence that bees visit flowers for the purpose of experiencing an aesthetic pleasure. Insects, especially bees, occasionally examine the neglected conspicuous flowers of cultivation; but, obtaining no food materials, or very little, they do not often repeat their visits. Many neglected flowers are pleasantly scented and the addition of another agreeable odor is neither necessary nor beneficial. When odoriferous fruit syrups are introduced into conspicuous flowers, commonly neglected, a group of miscellaneous insects, especially Diptera, will be attracted; but the inference that, therefore, color is no advantage and an agreeable odor is required is fallacious. For the introduction of an odorless syrup into similar flowers will induce insect visits in large numbers; also when flowers, with the nectar inaccessible to honey bees and, consequently, seldom visited by them, have the nectaries artificially punctured, or the floral tubes shortened by drought, they are then visited by bees in countless thousands without the addition of either an agreeable odor or a sweet liquid. Flowers which in one locality freely secrete nectar and are visited by numerous insects are sometimes in other localities nectarless and almost entirely neglected. Insects, therefore, perceive the colors and forms of neglected flowers, and the rarity of their visit is the result of their memory of the absence of food materials, not because the flowers lack an agreeable odor, which is often not the fact. The flowers into which Plateau introduced odoriferous sweet liquids were thus artificially converted into distinct physiological varieties. Since flowers possessing conspicuousness, an agreeable odor, and a liquid food are opposed to flowers possessing only conspicuousness, it is clear that color was never brought into competition with odor—the latter was invariably given the advantage. Colors and odors attract the attention of insects, but bees in their visits to flowers previously examined by them, are guided largely by the memory of past experience; they are able to associate different sense impressions and unconsciously make analogous inferences."

MATING INSTINCTS

McDermott (55) gives a resumé of the literature showing that in the Annelid worms and in the Lampyrid beetles phosphorescence is a mating behavior. He relates that the habits of the phosphorescent Elaterid genera *Pyrophorus* and *Photoporus* are unknown; and that *Bolitophila luminosa* is the only known self-luminous fly.

According to King (47), the littoral mite, *Gamasus immanis* Berl., mates the latter part of August, in a manner similar to that recorded for *G. terribilis* by Michael in 1886.

Triggerson (85) observed that the male of *Dryophanta ericacea* begins courtship by striking the female several times with his antennae. These taps quiet the female and render her submissive.

NEST BUILDING AND MATERNAL INSTINCTS

Detailed descriptions of the nesting and maternal habits of the mason bees of his part of France are given by Fabre (23).*

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* The small space given to the discussion of Fabre's work is due not to a lack of appreciation for him on the part of the reviewer; but to the fact that these articles were originally published, in the French, several years ago, and it is believed that most students of animal behavior are familiar with them in the original. The reading of all of Fabre's works will well repay any student of animal behavior. One will not always agree with his interpretation of the facts; for, to the day of his death, he was uncompromisingly opposed to the theory of evolution. He stands toward the animal behavior men of today in the same relation as did the elder Agassiz to the morphologists of his day. Agassiz had collected a wealth of material which he interpreted in terms of types of created things; but which his followers, assisted by additional researches, interpreted in terms of morphological evolution. Likewise Fabre has collected a wealth of material which he interprets in terms of preestablished and unchangeable instincts; but which his followers, assisted by additional researches, will interpret in terms of mental evolution.

Branch (10) states that *Entyla sinuata*, one of the Membracidae, oviposits in a slit which the female makes in the midrib of the underside of the leaf of the thistle *Cnicus altissimus.*

Weiss (99) informs us that the nest of *Paratenodera sinensis* consists of a horny core containing eggs, surrounded by a rind, which undoubtedly protects the egg from moisture and from sudden changes of temperature. By a long series of tests, he proved that the eggs were not subject to sudden changes of temperature.

Williams (102) has given us the following interesting facts about the behavior of certain Hymenoptera. The nest of *Mimesa argentifrons* is a vertical funnel surmounted by a frail cone of agglutinated grains of sand. *Priononyx thomae* Fab. deposits her prey in a place of safety while she constructs her one-celled burrow. *Priononyx atrata* St. Farg. digs its burrow with jaws and forefeet. When the time comes to close the burrow, the wasp fills it by backing in and throwing in dirt at the same time. After using her clypeus and jaws as a packer or ram the wasp smoothes over the burrow with strokes of her feet and then covers it with bits of soil, sticks, etc.

In another paper (101), he informs us that the Larridae of Kansas are partial to sandy situations and that they almost invariably excavate their own nests. Occasionally they build in brambles; but the majority of the species mine in the ground. With the exception of *Miscophus*, the egg is placed transversely.

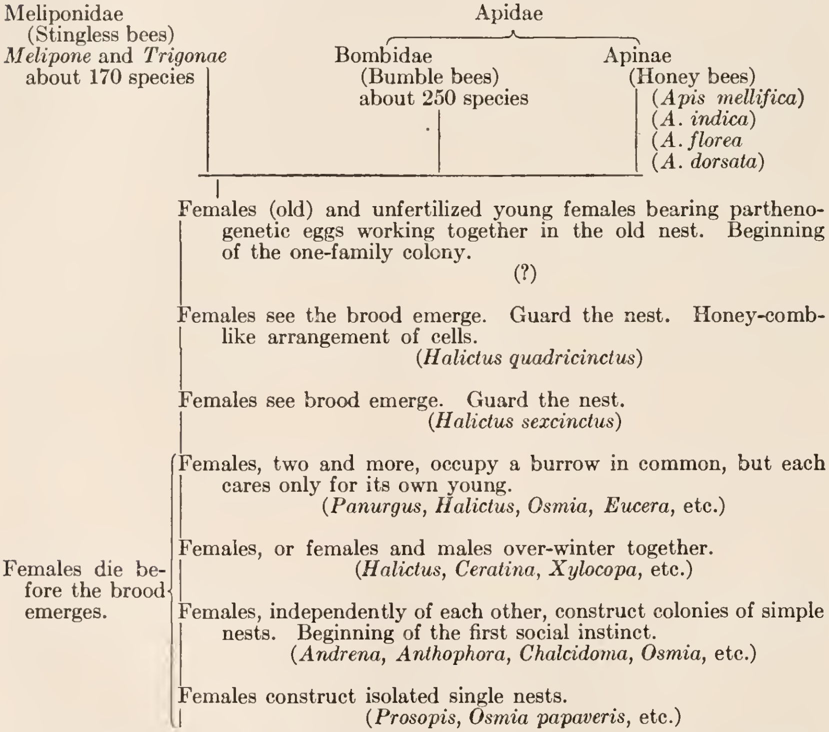
Strand (83) discusses the nest of an American Eumenid wasp and its inhabitants.

Dwight Isely (43) describes the nesting habits of six mining and two mason wasps of the family Eumenidae. The mining wasps belonged to the genus *Odynerus*. Each moistens the clay in which it is going to excavate with water brought, in periodic trips, from a nearby pond or stream; but, there is a marked contrast between their nests. The striking thing about many of these nests is the turret built around the entrance, out of a portion of the materials removed from the burrow. *Odynarus papagorum* Viereck constructs a turret which is durable under all ordinary conditions. The turret of *Odynerus arvensis* Sauss. is so frail that even a light rain destroys it. When the burrow of this species has been stocked, the wasp demolishes the turret and throws it, piece by piece, into the burrow. *Odynarus dor-*

salis Fab. does not construct a turret. *Odynarus annulatus* is variable in its habits. Sometimes it constructs a burrow with a turret and sometimes it uses an abandoned nest of *Pelopeus*. All of these burrowing species make locality studies before beginning their excavations. Many of these forms suspend the egg from the ceiling by a thread. Fabre thought this a device to prevent the egg from coming in contact with the squirming larvae. As Isely says, this cannot be true in all cases, for often the caterpillars are packed closely about the egg.

Beutel-Reepen (7) is convinced that the bees have ascended from forms having the habits of the digger wasps, and he crystallizes his opinion in the following table:

BIOLOGICAL ANCESTRAL TREE OF BEES



Meliponidae
(Stingless bees)
Melipone and *Trigonae*
about 170 species

Apidae

Bombidae
(Bumble bees)
about 250 species

Apinae
(Honey bees)
(*Apis mellifica*)
(*A. indica*)
(*A. florea*)
(*A. dorsata*)

Females (old) and unfertilized young females bearing parthenogenetic eggs working together in the old nest. Beginning of the one-family colony.
(?)

Females see the brood emerge. Guard the nest. Honey-comb-like arrangement of cells.
(*Halictus quadricinctus*)

Females see brood emerge. Guard the nest.
(*Halictus sexcinctus*)

Females die before the brood emerges.

Females, two and more, occupy a burrow in common, but each cares only for its own young.
(*Panurgus*, *Halictus*, *Osmia*, *Eucera*, etc.)

Females, or females and males over-winter together.
(*Halictus*, *Ceratina*, *Xylocopa*, etc.)

Females, independently of each other, construct colonies of simple nests. Beginning of the first social instinct.
(*Andrena*, *Anthophora*, *Chalcidoma*, *Osmia*, etc.)

Females construct isolated single nests.
(*Prosopis*, *Osmia papaveris*, etc.)

According to the Severins and Hartung (79), the melon fly (*Dacus curcubitae* Cog.) oviposits in the stem, petiole, flowers and fruit of pumpkins.

Herrick (33) reports that the apple pest (*Xylena antennata*) deposits its eggs in the leaf-scars before the leaves appear. He finds that, in confinement, *Ypsolophus pometellus* oviposits in the latter part of May.

Emery (22) discovered that *Simulium vittatum* breeds only in running water. The female attaches strings of 200 to 300 eggs to the rocks. There are three broods a year.

Bloeser (8) states that *Siphona plusiae* deposits one or more eggs on the outside of the Phrygnidian larva.

Welch (100) reports that the fly *Hydromyda confluens* Loew. constructs a gall on the submerged petiole of the water lily. He thinks the fly crawls down the stem and oviposits beneath the water.

Palmer (67) describes the laying habits of certain lady beetles.

FOOD PROCURING AND DEFENSIVE INSTINCTS

Campion (14) describes the feeding behavior of some dragon flies; Coad (16) of the boll-weevil and Houser (37) of *Conwentzia hageni*.

According to Bloeser (8) the larva of *Siphona plusiae* penetrates the body wall of the Phrygnidian larva and feeds upon its entrails. In ten days it is mature.

Branch (10) asserts that *Entylia sinuata*, a Membracid, feeds on the thistle (*Cnicus altissimus*).

By capturing flies and removing their prey from them, Bromley (11) has made a careful study of the food of eighteen species of Asilidae. He gives a list of prey that covers nearly six pages.

Emery (22) finds that the buffalo gnat (*Simulium vittatum*) will bite before ovipositing.

Girault (26) deprived half grown ant lion larvae of food for twenty-five days and found them still alive.

Guyénot (28A) found that the little fruit fly (*Drosophila ampeliophila*) develops normally when fed on sterilized yeast.

Heath (32) records an instance of a phalangid drinking milk.

The investigations of Hewitt (34) show that the dung fly *Scatophaga stercoraria* L. destroys large number of flies, especially Muscoid flies, by seizing the victim with its legs and piercing the neck with a thrust of the proboscis from below. After a moment's sucking, the fly is turned over and the proboscis thrust between the abdominal segments.

In another paper (35) he states that he could not get the stable fly to bite until twenty-four hours after emerging from the pupa. [Mitzmain got them to bite at the end of eight hours.] Usually it feeds not oftener than once in twenty-four hours.

Hueguenin (41) informs us that a Noctuid moth (*Heliothis dispaceus*), which feeds on the tar weed, also feeds on the larva of *Pontia rapae.*

Isely (43) reports that the young of all the Eumenidae studied by him feed on the plant-feeding larvae of other insects.

King (47) finds that the mite *Gemasus immanis* Berl. feeds on Oligochaetes. Truessart claims that *Molgus littoralis* feeds on *Collembola*; but King could not induce this mite to do so. He found it feeding on living Diptera.

Merrill (60) found a Clerid larva eating the caterpillars of the codling moth. Palmer (67) finds that the amount eaten by the larvae of the lady beetles varies with the weather and the size of the larva; and that the quantity eaten by the adult varies with the weather and the egg-laying activity. None feeds on vegetable matter; all eat plant lice.

The Severins and Hartung (79) find that the melon fly (*Dacus curcibitae*) feeds upon the cucumber, egg-plant, kohlrabi, muskmelon, pumpkin, squash, string bean, tomato, watermelon, wild cucurbit, mango, orange (?), and papaya. They feed from sunrise until 10 A. M., and rest during the hottest part of the day.

Venerables (91) finds the adult saw-fly *Tenthredo variegatus* feeds upon small Dipterous insects.

According to Welch (100) the young of *Hydromyza confluens* Loew. feeds on the waterlily.

Williams' investigations (103) show that the larva of the tiger beetle, *Amblychila cylindriformis* Say, comes to the surface at night and feeds on a variety of insects. It rejects distasteful ones, sometimes relinquishes an edible insect that it cannot subdue, and is occasionally overcome by its intended prey.

In another paper on solitary wasps, Williams (102) states that *Harpactus gyponae* stocks its nest with *Gyoina cineres*, a Jassid; that *Mimesa argentifrons* stocks its nest with *Athysanus exitiosa*, another Jassid; and that *Priononyx rufiventris* feeds its young on several species of short-horned grasshoppers.

In the following table Williams (101) has condensed much of the information gleaned from his investigations of the food habits of the Larridae of Kansas.

TABLE TO SHOW THE PREY OF THE LARRIDAE

Wasp	Prey	Order to which prey belongs
Larra americana	Gryllidae	Almost exclusively Orthoptera; the two exceptions are. Lepidoptera and Diptera.(*)
Larra anathema (Europe)	Mole crickets (Gryllidae)	
Notogonia agentata	Immature *Gryllus* (Gryllidae)	
Larropsis divisa	*Ceutophilus* sp. (Locustidae)	
Tachytes abdominalis	Immature Tettiginae and Acridiinae	
Tachytes distinctus	Various Melanopli, *M. ferum-rubrum*, usually immature; *Ageneotettix deorum*, mature (Acridiinea and Tryxalinae)	
Tachytes fulviventris	Mature *Alpha crenulata* (Tryxalinae)	
Tachytes harpax	*Niphidium brevipenne* (Locustidae)	
Tachytes mandibularis	*Niphidium* and im. *Orchelimum*	
Tachytes mergus	Immature Tettiginae	
Tachytes obductus	Immature Tettiginae	
Tachytes obsoletus (Europe)	Young Oedipodinae	
Tachytes pompiliformis (Europe)	Immature *Gryllus rufus*, grasshoppers (*Choiptusr*); lepidopterous larvae	
Tachytes rufofasciatus	Immature *Melanoplus cyanipes*, mature and immature Melanopli (Acridiinae)	
Tachytes tarsina (Europe)	Immature Acridiinae	
Tachysphex fusus	Immature Melanopli (Acridiinae)	
Tachysphex hitei	Immature Litaneutria minor (Mantidae)	
Tachysphex panzer (Europe)	Acridiinae	
Tachysphex plenoculiformis	Immature Traxalinae	
Trachysphex propinquus	Mature *Alpha crenulata*, *Ageneotettix deorum* and *Mestobregma kiowa*; immature *Opeia* sp. (Tryxalinae and Oedipodinae	
Tachysphea quebecensis	Immature Acridiinae	
Tachysphex semirufa	Immature *Melanoplus spretus*	
Tachysphex tarsatus	Immature Acridiinae, Tryxalinae and Oedipodinae	
Tachysphex terminatus	*Chortophaga viridifasciata*, immature Tryxalinae	
Trachysphex texanus	Immature Oedipodinae (Diptera)*.	
Lyorda subita	Nemobius; small crickets (*Gryllidae*)	
Plenoculus apicalis	Mature and immature Atomoscelis sp. (Capsidae)	Hemiptera. Exceptions: Orthoptera and Arachnida (†)
Plenoculus peckhami	Immature Pamera sp. (Lygaeidae).	
Niteliopsis fossor	Immature Oedipodinae†	
Niteliopsis inerme	Immature Capsidae	
Miscophus spp. (Europe)	Various small spiders, Epeiridae†	

Miss Alice Noyes (65) has made a careful study of the net-spinning Trichoptera of Cascadilla Creek. She confirms Siltala's statement that the food of *Hydropsyche* is both animal and vegetable. In the fall and winter diatoms form the bulk of their food; in the spring and summer animal food predominates. The members of the family Polycentropidae have an animal diet. *Chimamba alterrima* of the family Philopotamidae, feeds exclusively on plants. She makes the following conclusions:—1. Many forms construct their nets only at night; but Hydropsyche spins by either night or day. 2. Two and a half to three hours is the average time required to weave a net. 3. Different species build similar dwellings. 4. Unlike the orb-weaving spiders, they do not spin preliminary construction threads. 5. There is no definite order followed in spinning the threads. 6. The mouthparts, not the tufts of hair on the anal legs, are used to remove particles from the nets. 7. Its front legs and mandibles are used for seizing and holding in place until fastened any bits that the insect intends to weave into the net. 8. It is never too intent on its weaving to pick up bits of food that become entangled in the nets. 9. Food captured in the net is dragged inward, killed and swallowed whole.

HIBERNATION

Houser (37) states that *Conwentzia hageni* hibernates in the larval stage.

Palmer (67) says all lady beetles hibernate in the adult form.

F. E. Lillie (48) thinks that some individuals of the monarch butterfly hibernate in the adult form, although she could not obtain confirmatory evidence of this. Indeed, all attempts to induce them to hibernate by keeping them in a cool, dark place failed; but in May she found adults with unfrayed wings.

King (47) does not say that the mite *Bdella longicornis* hibernates; but he states that it constructs a web in which it apparently spends the winter.

Baumberger (4) reviews, at length, the literature on hibernation of insects and reaches the following conclusion:—" 1. That temperature is but a single factor and not necessarily the controlling one in hibernation. 2. That hibernation is usually concomitant with overfeeding and may be the result of that condition or the result of accumulation of inactive substances in the cytoplasm of the cell due to the feeding on innutritive food. 3. That the loss of water which is general in hibernation probably results in a discharge of insoluble alveolar cytoplasmic structures which have accumulated and produced premature senility with an accompanying lowering of the rate of metabolic processes. 4. That starvation during hibernation together with this loss of water may result in rejuvenation, when aided by histolysis, and in increased permeability. 5. That this rejuvenated condition and increased permeability, will, if stimulated to activity by heat, permit pupation in codling moth larvae, which in this case is the termination of the hibernating condition."

LOCOMOTION

Branch (10) relates that *Entyla sinuata*, a Membracid studied by him, exhibits no jumping activities in its nymphal stages.

The Severins and Hartung (79) tell us that the nearly full-grown larvae of the melon fly (*Dacus curcubitae* Coq.) exhibit a jumping activity which is never seen in the younger stages. The insect curls its body into a circle with its jaws attached to the tip of its abdomen. Then, by suddenly relaxing, it springs six or eight inches into the air.

Becker (5) describes a rather interesting migratory procession of the larvae of the fungus gnat (*Sciara congregata* Johannsen). Such a procession was observed June 6, 1912 and again July 16, 1913. At first glance it resembled a dead snake. The procession observed in 1912 was five feet long and tapered toward each end from the middle, which was three inches wide. There were several layers of larvae, tapering from eight deep in the middle to each extremity. The whole procession was moving forward; but the maggots on top moved much faster than those on the bottom, hence the insects of the upper layers were constantly advancing beyond the front, to be immediately covered by others coming from the rear. In its wake the procession left a trail resembling that of a snake. The procession of 1913 was only three feet long.

ECOLOGY

By exposing laboratory animals to controlled stimuli similar to those of their natural habitats, Shelford (81) has been able to analyze the behavior of animals forming communities and has reached the following conclusions:—" 1. The animals of a community are in agreement in the reaction to certain intensities of two or more factors. These reactions may be used to designate them. Thus the rapids community may be designated as litho-rheotactic, meaning that the animals are arranged with reference to current and stones of considerable size. 2. Animals living in the same or comparable situations within the community habitat are in agreement and the animals of different situations react differently to these additional factors. Similar differences are the physiological basis of strata and consocies though the smaller number of species make the latter not easily distinguishable here. 3. Single species found in any community occur in other situations where they are governed chiefly by stimuli towards which there is not agreement of reaction throughout the community to which they primarily belong."

Wolcott (104) discusses the ecology of the parasitic wasp *Tiphia inornata.*

See McDermott under mating instincts.

DISEASE SPREADING ACTIVITIES

The papers of Graham-Smith (27), Harms (30), Lloyd (49,50), Ludlow (54), Riley (74) and Webster (95) will be of interest to physicians.

Zetek (106) has demonstrated that the typhoid fever spreading fly visits houses 2,500 feet away from the feeding places of its larvae.

Three theories have been proposed to account for the spread of pellagra: (1) the zeistic theory, based on the work of Ballardini, which appeared in 1895, which claims that it is a poisoning due to the excessive use of the products of corn; (2) Mizell's theory, proposed in 1911, which holds that it is poisoning due to the use of cotton-seed products; Sambon's theory, dating from 1910, which holds that it is spread by the sand-fly. Sambon bases his theory upon the following statements: (1) the endemic action in Italy has remained the same since the disease first appeared; (2) the season of recurrence coincides with and fluctuates with the season of the appearance of the adult sand-fly: (3) in the center of infection whole families are attacked simultaneously; (4) in non-pellagrous districts the disease never spreads to others on the advent of pellagrins; (5) in families moving into non-pellagrous districts, children born in the former district are pellagrous, while others are not; (6) the disease is not hereditary; (7) it is not contagious. The advent of pellagra in Kansas gave Hunter (42) an opportunity to conduct a series of experiments which yielded the following results:—"(1) the number of sand-flies is directly proportional to the number of cases of pellagra; (2) the appearance of the cases of pellagra is coincident with the principal broods; (3) just succeeding the time of the principal broods the flies seem to bite more vigorously; (4) sand-flies which have fed on human blood live several days longer than those which have not been so nourished, thus favoring an incubation theory for a parasite, if such there be; (5) pellagra, thus far in Kansas, has appeared almost entirely in one restricted locality; of nine cases recorded last year five were traced back to one town; in this region flies are widely distributed; (6) no direct evidence has yet been found which would in any way warrant any conclusion with reference to an association of the sand-fly in the determination of the etiology of pellagra." Hunter hopes to continue his researches until the problem is solved.

PARASITISM

Cummins (18) describes a sarcoptid mite of the cat; Howard (37) several mites of the gypsy moth; and Bloeser (8) a hymenopterous hyper-parasite of *Siphona plusiae* Coq.

Triggerson (85) gives a list of the numerous parasites of *Dryophanta erinacei.*

Isely (43) tells us that the Eumenidae of Kansas are parasitized by the Bombilidae, the Tachinidae, the Ichneumonidae, the Braconidae, the Mutilidae, the Myrmicidae and the Asilidae; but that the most persistent parasites belong to the Chrysididae. He believes that the turrets constructed about the entrances of so many burrows of this group of insects are to prevent the entrance of parasites.

Muir (61) discusses the effect of parasites on the struggle for existence.

Fabre (23) describes the parasites of the mason-bees of his part of France and states that he does not believe that parasitic habits result from a love of inactivity; for he finds that parasites work hard. The parasite *Stelis* must break through walls as hard as concrete to deposit its eggs.

Waterson (94) discusses the bird-lice of five species of English auks. In addition to a parasite peculiar to it, each often has one or more other parasites. He has epitomized his conclusions in the following table.

	Uria troile	*Alca torda*	*Fratercula arctica*	*Uria (Cepphus) grylle*	*Mergulus alle*	*Rissa tridactyla*
	11 birds	6 birds	6 birds	11 birds	10 birds	1 bird
D. acutipectus, Kell....	....	....	1{ x (6)	....	....	....
D. calvus, Kell........	2{ x (11)	....	1{ x (2)	....	....	x (S)
D. celedoxus, N........	1{ x (2)	1{ x(6)	x(2)	....	....	Solitary female with numerous examples of *D. lari*, D.
D. megacephalus, D....		x(1) (S)	....	x(11)	....	
D. merguli, D.........	x(1) (S)	....	....	....	x(10)	
D. icterodes, N........	....	x(1)(S)	....	....	1{	....
D. cardiceps, P........	....	....	....	....	x(1)(S)	....

An x denotes the occurrence of *Docophorus* on bird species. The number in brackets indicates how often the parasite has occurred on the host species.

The long brackets with their numbers show how often and how many species have occurred together on an individual host.

(S) = straggler.

Thus, column one reads: "Of *Uria troile*, 11 birds have been examined and on all *D. calvus* has been taken. In two cases *D. calvus* has been found with *D. celedoxus*, and once with *D. merguli*. This last, however, seems a case of straggling.

SOUND PRODUCING ACTIVITIES

Regen (72) mentions the enticement of the female of a cricket (*Gryllus campestris*) by the stridulations of the male.

Mrs. Comstock (17) discusses, in a popular vein, the stridulations of crickets and gives directions for keeping the insects in confinement.

Aubin (2) has performed some experiments which cause him to assert that the high-pitched note produced by flies is due neither to the vibrations of the wings, nor to the pulsations of the thorax, nor to special modifications of the occlusor apparatus of the stigmata, nor to movements of the halteres; but, to a special sound-producing apparatus which is situated, on each side of the thorax, near the base of the wings. This consists of a membrane-lined depression which is traversed diagonally by two ridges or ribs. When the wings are vibrated rapidly a chitinous structure on the base of each strikes against one of the ridges of the apparatus just described and induces the membrane to vibrate. This produces the high pitched sound we call buzzing. The following results of his experiments on the drone-fly (*Eristalis tenax*) seem to justify his conclusion:—1. The fly may be held in the fingers in any way but one without appreciably affecting the buzzing; press the shoulders of the wings to the body and the buzzing ceases immediately. 2. Each or all of the following parts may be removed without noticeably affecting the sound; halteres, squama, ante-squama, aulet and nine-tenths of the wing. If the entire wing is removed the sound ceases. 3. If the vibrations of the aulets be checked by a needle applied at the convexity of the chitinous part, the sound continues. 4. If, while the fly is buzzing, a needle is inserted between the chitinous part of the wing base and the ribs on the body of the fly, although the parts are uninjured, the buzzing ceases. 5. A minute spear of tissue paper inserted between the chitinous portion of the base of the wing and the ribs on the thorax subdues the sound. 6. If pins be so placed that, without injuring the wings, they prevent them from closing closer than 45° with the axis of the body, no buzzing is heard. Evidently this organ is of a higher order than the stridulating apparatus of the Orthoptera. Since a portion of it consists of a vibrating membrane, Aubin is inclined to believe that it is a sound-receptor as well as a sound-producer.

In the discussion which followed the reading of this paper before the Royal Microscopical Society of London, a Mr. Hopkins arose and stated that, eighty years ago, Burmeister had performed similar experiments which yielded identical results, and he wondered why Aubin had not cited Burmeister's experiments. At this writing, Burmeister's original paper is not accessible to the present writer; but, judging from the quotation made at the meeting of the society, Burmeister held that certain folds connected with the spiracles were the cause of the sound. Unless the reviewer entirely misunderstands Aubin's words and illustrations, his apparatus is not connected with the spiracles. It does, however, seem strange that Aubin makes no mention of the work of Pemberton* which was reviewed in this journal about three years ago†. Pemberton, as a result of his experiments, insisted that there is no spiracular voice in insects and that the high-pitched notes of the Diptera and the Hymenoptera are caused by the striking of the bases of the vibrating wings against the sides of the thorax. Apparently he overlooked the ridges against which the wings impinge and the vibratile membranes connected with these ridges.

DURATION OF LIFE

Baumberger (4) found that, when exposed to constant temperatures, the longevity of insects varies, approximately, inversely with the temperature; when exposed to variable temperatures, a high or low temperature followed by medium temperatures favors a lengthening of life; exposure to a medium temperature at the beginning shortens life.

By crossing a short-lived strain of the fruit-fly (*Drosophila ampelophila*) with a long-lived strain, offspring were obtained which were longer lived than either of the parents. In the second generation some reverted to the short-lived condition. He thinks there is a physiological connection between the length of life and the coming into maturity of the germ cells.

Sanderson and Peairs (76) give a table showing the relation of temperature to the duration of life. The table was compiled from more than 400 separate experiments involving 390,000 individuals.

Phillips and Demuth (70) assert that the length of life of bees varies inversely with the amount of work they do; hence, to secure vigorous bees for the spring, the work to be done in winter should be reduced to a minimum.

Phil and Nellie Rau (71) have made extended studies of the longevity of the following Saturnid moths: *Philosamia cynthia*, *Telea Polyphemus*, *Callosamia promethea*, *Samia californica*, and

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* Pemberton, C. E. Sound Producing Diptera and Hymenoptera. *Psyche*, vol. 18, pp. 82-83, 1911.

† *Jour. of Animal Behav.*, vol. 2, p. 396, 1912.

Samia cecropia. They tested both mated and unmated individuals. In all they studied 3,569 individuals. The following are some of their conclusions: (1) mating does not significantly shorten or lengthen the life of the male; (2) the unmated female lives longer than the mated: (3) a low temperature lengthens the life of *S. cecropia* and of *T. polyphemus.*

MISCELLANEOUS

Shelford (81) discusses the importance of evaporation in insect behavior.

1. *Migration.* Lillie (48) records the well known fact that the monarch butterflies of Minnesota and of New York migrate southward in vast swarms each fall; and Davidson (19) discusses the migration of certain plant lice.

See Becker under locomotion.

2. *Pain.* Weiss (97) reviews the arguments that have been produced as proof that insects do not feel pain and concludes that "The evidence for assuming that insects do not suffer acute pain is not by any means complete. We simply do not know and have no reliable means at present of finding out."

3. *Pollenization.* Mrs. Howard (38) has experimentally proven that bees are needed to pollenize certain plants. She covered 100 clover blossoms with netting and left 100 exposed to the bees. From the uncovered blossoms she obtained 2,720 seed; none of the covered blossoms produced seed. Of 2,586 covered apple blossoms only three matured.

4. *Sleeping habits.* According to Beutel-Reepen (7) the males of several species of solitary bees spend the night congregated in large clusters.

Williams (102) states that large groups of the males of the wasp *Priononyx thomae* spend the nights and unfavorable weather on the weeds.

Frohawk (24) describes the sleeping habits of the butterflies of the family Lyncaenidae.

5. *Temperature.* In an extended study of the temperature of the bee-hive, Gates (25) discovered that, even in cold weather, the bees are neither torpid nor semi-quiescent. There is a constant interchange of individuals between the outside and the inside of the cluster. Even in the coldest weather, they groom and comb one another.

MEMORY AND ASSOCIATION

Beutel-Reepen (6) believes that bees have a memory picture of the environment which guides them home.

Fabre (23) insists that it is not memory, but, a special sense which guides insects home.

Hudson (39) gives a short note on the memory of a Pompilid wasp.

Lovell (51) says; "Experiment and studies of the honey-plants show that honey-bees learn from observation and are guided by the memory of past experience. Flowers rich in accessible food supplies receive numerous visits, but if for any reason the flow of nectar suddenly ceases the bees immediately discontinue their visits."

See Lovell under visual sensations.

TECHNIQUE

O'Kane (66) describes the construction of an outdoor insectary, with a conservatory roof, which has screen sides for summer and glass sides for winter.

Shelford (8) discusses the importance of using atommeters in studying insect behavior.

Wolff (105) discusses methods of investigating the temperature reactions of butterflies.

Mrs. Comstock (17) redescribes a method, well known to students of insects, of keeping crickets in large lamp chimmeys resting in flower pots.

Phillips and Demuth (70) describe a method of investigating bee hives by means of electrical thermometers.

Draper (20) has devised a convenient live box for studying insects under the lowest powers of the microscope. A piece of glass tubing one-third of an inch deep and two-thirds of an inch in diameter is cemented to a standard microscopic slide. A circular piece of glass, of larger diameter than the cell, serves as a cover. Near the circumference of this cover and equispaced, three pins are attached to the underside. The collar by which each pin is attached to the cover prevents it from touching the top of the cell and thus insures ventilation. A false bottom permits regulation of the depth of the cell.

Baumberger (4) describes a convenient net for securing large quantities of live insects. The net is constructed in the following manner: "A strong piece of iron wire, three feet, eight inches long, is bent into a circle with one foot diameter—the ends are then bent at right engles so as to lie adjacent and parallel to each other. The ends are inserted into the small end of a six inch ferule and soldered fast. A short two foot handle will be found best for sweeping. The net consists of white muslin—a conical bag about eighteen inches deep. The tip is cut off where the circumference of the bag measures about three inches and is replaced by a cloth bag four by six and a half inches. This small bag is sewed to the point at which the circumference of the large net is four inches, thus, leaving a sleeve which hangs down into the small bag—this small bag will just hold a quarter pound paper bag. The sleeve of the large net fits into the paper bag. When filled from a minute's sweeping, the paper bag is pinched at the opening, taken out of the net and placed in a botanical can. Upon the return to the laboratory the bag is opened at a well lighted window and the contents picked over for specimens."

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LITERATURE FOR 1914 ON THE BEHAVIOR OF VERTEBRATES

STELLA B. VINCENT
Chicago Normal College

VISION

Mammals.—Both Bingham (3) and Johnson (15) reply to Hunter's criticism of last year, in which he insists that form must be considered as a part of a pattern—that the stimulus object is seen against a visual background. The former urges first, that his apparatus was in a dark room which enabled him to control conditions of setting and second, that a distinction must be made between form and shape, i. e., two similar triangles, the one upright and the other inverted, would be alike in form but differ in shape. In his experiments he got a relatively low per cent of correct choices when the triangle was inverted; obviously, then, form was not the basis of choice. The perception of shape, for Bingham, is based upon an unequal stimulation of different parts of the retina.

Johnson argues that to change, as Hunter suggests, the alleys in the Yerkes' experiment box to hollow cylinders or triangles would also change the tactual and probably the olfactory qualities. Admitting that the background changes according as the stimulus object occupies the right or the left position respectively, he insists that if the stimulus form is as effective in one setting as in the other the conclusion is justified that the animal is really reacting to the constant form difference and disregarding the variable " pattern " difference.

The best articles on vision in the year 1914 are those of Johnson, beginning his series on Pattern Discrimination in Vertebrates (16), (17). The first paper deals with the standard methods of studying vision, the elementary problems in pattern discrimination and the apparatus and methods. The author defines pattern discrimination as a discrimination between visual fields equal in outline, area and average brightness and differing only in the disposition of their brightness. Four problems are then outlined: (1) the stimulus threshold for striation, i. e., the width of the individual bands on one field to insure discrimination between it and another sensibly uniform field; (2) the threshold difference for size and conversely for number of individual bands; (3) the difference threshold for direction of bands; (4) the difference threshold for contrast which is really the threshold for brightness.

To produce the pattern two glasses were ruled with fine opaque lines so that the width of the lines and the clear spaces were equal. When these plates were rotated over each other there was shown a series of dark and bright bands of equal width. It is Cobb's apparatus slightly changed. With this was used a modification of the Yerkes—Watson double photometer box. The whole apparatus is excellent in conception and control, but for the exact details the reader must consult the original papers.

In attacking the first problem mentioned above, Johnson used a series of black and white horizontal bands of equal width with respect to each other but whose absolute width could be varied from one which was invisible to one which was plainly visible without changing any other factor. The task set was to distinguish this pattern field from a plain field having the same area, form, range of wave lengths and luminous intensity. The subjects used were dogs, monkeys and chicks. The details of the experiments are full of interest. No positive results were obtained from the dog as his behavior showed no sensitiveness to differences of detail in visual objects. The experimenter, however, reserves his conclusions on this subject. The visual acuity of the monkey was very like that of the human subjects used for comparison. The acuity of the chick was about one-fourth that of the monkey. The author raised further questions as to the method of experimentation the most significant of which, it seemed to the reviewer, was that relating to the optimal distance of the test field from the animal. The distance used in the work reported was, uniformly, 60 cm.

In some experimentation not nearly so well controlled as to the different factors involved, Szymanski (35) reports some work on the learning process in white rats. The apparatus consisted of three connected parts, but, as parts one and two were so very simple and gave such inconclusive results they may be neglected. In the third part, the animals were required to go to the right or to the left according to a visual clue. Lamps of 10 and 2 c. p. were used as stimuli. The temperature was controlled. After 30 trials punishment was introduced and after 50 trials the 2 cp. lamp was discarded and the discrimination was made between the 10 c. p. light and darkness. The rats had from 93 to 109 trials and only 2 out of 14 animals learned to follow the light. These two animals made percentages ranging from 60 to 100. In five other instances the percentages were rising, although the fact is not mentioned, and probably further trials might have trained these also as Foley's work with sparrows, reported below, shows. The work was stopped too soon. One animal persistently chose the dark way. Many set up position habits. Numbers 4, 5, 13 and 14 went three times as often to the left as to the right and number 12 went four times as often to the right as to the left.

" The understanding of an organism " the same author says, (34) " is won through an analysis of its motor activities." This analysis falls into two chief parts: first, the conditions which influence the general motor activity positively or negatively—conditions which may be outside or may lie within the organism and second, the study of the organized movements going on under the influence of determined and directed stimuli. " In reality," he says, "the course has been reversed and investigators have begun with a study of organized activities and only here and there have sought to clear up the variability of the reaction to a determined stimulus by reference to the changing conditions of the organism or of the environment especially the temporal environment." By means of a bit of apparatus which is called an aktograph, he records the activities of a series of animals through the entire course of a series of days including days chosen at different times of the year. The apparatus was so arranged as to connect with a Marey tambour and make a kymograph tracing. Among other animals white and gray mice were used. No division of activities dependent upon light and darkness was seen in these animals. The white mice showed 16 periods of activity averaging 45 minutes each, the gray 19 such periods averaging 37.9 minutes each. These periods were fairly evenly distributed between night and day. He says, "It appears as if the division of rest and activity periods should depend upon the preponderance of this or that sense in the life of the species. If the eye plays the chief rôle in the life of the species it is light which regulates the relations of rest and activity; if other senses have the preponderance, smell, hearing, kinaesthesis, these are the factors which lead to a different division."

Amphibians.—Laurens (19) investigated the reactions of normal and eyeless amphibian larvae to light. The forms which he used were *Rana pipiens*, *R. sylvatica* and *Amblystoma punctatus*. Neither normal nor blinded frog tadpoles gave any response to light stimulation. The behavior of Amblystoma, however, was different. Both normal and blinded individuals were found to be positively phototropic. These reactions, as proved by operative surgery, were not due to direct stimulation of the central nervous system but by stimulation of nerve terminals in the skin.

Fish.—More work on the color adaptations of fish has been done by Freytag (6). He used *Phoxinus laevis*, von Frisch's best subject, and tried it over many backgrounds. His experiments lasted a year and he used in all 100 animals. These animals were described minutely before and after the experiments and sometimes they were kept for 24 hours on the same background. There were always control animals. As a result of this work Freytag thinks that the adaptations are to brightness and not to color at all. There is no doubt he says that the brightness affects the animals, but no law can be formulated. Not infrequently was there no change and quite as frequently was the change in the opposite direction from what was to be expected. The yellow and red markings showed very inconstant changes and never was yellow or red the rule. On the other hand these colors were often seen when the fish were over other colors or over gray. He thinks the changes which do occur are not a response to background alone but are due to other conditions as well. The enemies of the fish do not all come from above. It is quite as possible that the adaptation may be to the brightness of the water in which it swims. None of the work speaks for a color sense for Pfrille.

Two other investigators (9) in the main confirm the findings of von Frisch.

The aktograph records of Szymanski (34) showed that the periods of activity of the fish were regulated by light. They began about one hour before sunrise and ended about one and one-half hour after sunset.

Birds.—Light discrimination in the English sparrow was the object of Tugman's (37) experiments and the main attempt was to find the threshold of brightness. The Yerkes-Watson brightness apparatus and experiment box was used. The birds were kept in darkness before the beginning of the work. Correct choices for two days (30 trials) were counted as a correct discrimination and then the difference between the standard .098 and the variable was decreased and the tests were resumed. The discrimination differences attempted ranged from .096 to .009 and the estimated thresholds for four birds were, .015, .035, .03, .022 c. p. The estimated thresholds for some human subjects were .013, .009, .013 c. p. The author says, "One of the most striking facts is the very large number of trials necessary to bring the animal to the threshold. The three animals for which the threshold was determined averaged 2,420 trials each. For the discrimination of the lowest threshold they averaged 480 trials each; one of them discriminated only after 615 trials.

As will be seen we are at last getting some well controlled work on sensory discrimination in animals. The first of the studies from the Franklin Field Station which Yerkes describes in the same journal (41) is reported by Coburn (4). He gives a preliminary study of the crow's ability to discriminate brightness, size and form. In the investigation he used but two birds. The apparatus was a modified form of the discrimination box used by Breed and Cole in their study of the visual reactions of chicks and the stimulus plates were the standard plates of the Yerkes-Watson apparatus. Both method and apparatus are carefully described in the report. The crows learned to discriminate between an opal flashed glass and an opal flashed glass backed first by two milk glasses, second, by one milk glass; third, by a sheet of paper. These roughly indicate the crows' ability to distinguish differences in illumination. Then they learned to discriminate between a 9 cm. and a 2 cm. circle; between a 5 cm. and a 3 cm. circle and between a 3 cm., and a 2 cm. circle. Another series of experiments, however, showed that the crows were reacting not to absolute size but to relative, i. e. to the larger circle. They also learned to distinguish a circle from a triangle and from a hexagon under conditions where form was the only constant factor.

Szymanski's (34) aktograph curve for birds shows a high degree of activity for the morning hours. In the hours immediately after noon the activity decreased and then with great constancy rose again just before the beginning of the night rest. In this respect it resembles the waking (day) curve of men given by Helpach. The movements of the birds can be inhibited not entirely but greatly in intensity and also in general nature by darkness.

SOUND

Mammals.—There are only three papers to report on auditory sensitivity in connection with vertebrates this year. Shepherd (31) using the method which he had previously employed with raccoons and monkeys investigated sound discrimination in cats. The notes were blown on an ordinary harmonica or sounded on a piano. The animals were to climb up in a cage for food at one note or refrain from climbing when another was sounded. He thinks that he found positive proof of pitch discrimination as well as discrimination of intensity in noise. The constant presence of the experimenter makes possible other clues and renders these results doubtful as Johnson's work with dogs has shown.

Considering the work mentioned above and other similar studies the following report by Morgulis (23) seems almost incredible yet perhaps our apparatus or methods are at fault. He discusses clearly and concisely the Pawlow method and makes a brief report on Usiewitch's study of the auditory reactions of the dog by this method. He found an auditory faculty much keener than man's. The dog perceived one-eighth of a tone and tones of a frequency of vibration quite beyond human reach. He found absolute memory for sounds and his dogs could distinguish the shortening of an interval by less than one-fortieth to one-forty-third of a second.

Hunter (12) has less startling results in a report which he makes upon some studies of the auditory sensitivity of the white rat which are now in progress at the University of Texas.

A 512 c′ fork was used for the tone stimulus and the alternate noise stimulus was hand clapping. The animals were to go to the right or to the left respectively at the stimulus. Under the conditions the errors steadily decreased but when the tone was withheld the reaction remained the same proving that the animals were reacting to noise only. None of seven rats in 700 trials learned to react to tone. There was also failure to discriminate between different intensities of c′. Many noises were substituted successfully for hand clapping with another set of animals. Interrupted tones gave no better results. The author concludes that either the rats cannot hear c′ or that their sensitivity to this tone is extremely slight.

OLFACTION AND CHEMICAL SENSITIVITY

Mammals.—A note on the supposed hunting response of the dog by Johnson (15) is a brief but interesting bit of analysis of some of the theories adduced to account for the ability of the dog to trail his master or track his prey.

Amphibians.—Risser (27) studied the toad in its reactions to natural or artificial food odor in living or dead animals. Odor streams were tried and the experiments were repeated in darkness. The species used was *Bufo americanus* Le Conte. His conclusions are that the visual is the sole stimulus which arouses the food response of the toad and that this is effective only when the food is in motion. Rejection of food, he thinks, is due to mechanical or tactual stimulation and the gustatory function is negligible. No positive conclusions are drawn as to olfaction yet he found it difficult to establish any connection between the seeking of food and inherent food odors. Odor streams caused definite motor activities which were proven by operative procedure to have an olfactory stimulus. The reactions which were inhibited by section of the olfactory tract were not affected by section of the trigeminal nerve. Tadpoles appear to use olfaction in discrimination of foods. The author says, "In the metamorphosed toad the visual stimulus is the principal and guiding factor in procuring food. Therefore it is inhibitory in relation to other stimuli and their resultant reactions."

Rapid modification of behavior was noted by Shelford (30) in experiments designed to test the sensibility of amphibians to variations in the evaporating power of air. In this case the conditions were such as would tend to dilute or concentrate the plasma either in the peripheral sense organs or in the animal as a whole. Though associations are formed, Shelford says, they go hand in hand with and can hardly be distinguished from other type modifications * * * There is no reason to assume that associative memory is essentially different from the type of modification here described. * * * It seems probable that many of the simple problems of associative memory must be referred to the bio-chemist for solution."

Fish.—Shelford (29) also has published an elaborate bit of experimentation with different kinds of fish. They exhibited distinct differences in behavior in the presence of modified water. He inclines to the belief that the changes in activity are due to physiological conditions in many cases connected with CO_2 relations and do not necessarily involve associative memory at all. The stimuli which gave rise to the modifications most quickly are those most commonly encountered by the fish in water—a disturbance of neutrality either in the direction of acidity or alkalinity.

INSTINCTS AND HABITS

Mammals.—In the hope of coming to a better understanding of abnormal human sexual behavior, Hamilton (10) has made an excellent study of sexual behavior in monkeys. The observations included twenty animals mature, immature and eunuchs and were made under the varied conditions of confinement and free range which a California laboratory permits. For the many suggestive details the original paper must be consulted. The author regards behavior as an expression of reactive tendencies which have specific representation in structure. He says, "The essential factors in the behavior phenomena are (a) the action of a physiological process usually operating in conjunction with environmental forces, in the production of (b) hungers which impel the individual to manifest (c) activities, the particular types or modes of which are to be ascribed to (d) specific organic properties (reactive tendencies)." He recognizes three hungers which normally impel the macaque to manifest sexual behavior, viz: hunger for sexual satisfaction, hunger for escape from danger and possibly hunger for access to an enemy.

Lashley (18) gives us a note on the persistence of an instinct and Hahn (8) a popular account of the hibernation of certain animals. Among them are the bear, woodchuck and ground squirrel.

Amphibians.—The only article on the instinctive behavior of amphibians is that of Banta (1) who gives some interesting notes and careful observations of the mating behavior of wood frogs which he watched at Cut off Pond, Cold Springs Harbor.

Birds.—To observe the very first performance of the social activities of an adult one must rear the animal in isolation and then allow it, while under close observation, to come in contact with another animal. Craig (5) does this and gives an interesting account of the behavior of three male doves which he has thus brought up apart from their fellows. He concludes, among other things, that display behavior needs social stimulation; that the motor aspect of the sexual reaction is definitely provided for by inheritance but that what he calls the 'sensory inlet' is not complete and is supplemented by experience; and that one finds surprise, hesitation and even fear in the first performance of an instinctive act while ease, skill and intelligent adaptation are the gift of experience.

Another paper which deals with sexual reactions also is that of Huxley (13). He recounts in elaborate detail the very dramatic courtship or love habits of the Grebe. After describing the bird and giving its animal history, he turns to the real interest of the study—the relation of the sexes. (a) in the act of pairing, (b) courtship, (c) nest building, (d) the relation of different pairs, (e) other activities. In the discussion which follows, the author tries to explain what he calls the facultative reversal of the sexes in the act of pairing—a subject which is interesting many investigators at the present time—and he then elaborates a modification of the sexual selection theory which he calls mutual selection. He says, " Where combined courtship actions exist and a variation in the direction of bright color or strange structure occurred it would make the actions more exciting and enjoyable and those birds which showed the new variation first would pair up first and peg out their 'territories' for nesting before the others could get mates." * * * As to the courtship activities he says, " These actions are much too elaborate and much too specialized to be considered as the immediate outcome of any form of physiological excitement. They obviously have a long and complicated evolution behind them and as they can only be performed by two birds together there is nothing to account for them as they now stand but some such process as I have just sketched under the name of mutual selection. The second part of the paper gives in detail some of the material worked up in the first as well as some notes on various points not connected with the main interest of the study.

Pearl (26) gives us, in the seventh paper of his series, a discussion of the brooding instinct which is very much changed possibly under domestication and certainly very much curtailed under the methods employed at the Agricultural Experiment Station. He concludes, however, (1) that it occurs with greater or less regularity following periods of egg laying. (2) that it varies in intensity at different times in the same individual. (3) that it is not necessarily connected with any season and may occur out of breeding season. (4) that it is ordinarily but not necessarily preceded by the laying of a clutch of eggs. (5) that it is apparently closely connected with the functions of the ovary although the precise nature of the connection has not yet been analyzed.

The flocking habits of migratory birds is the subject of Trowbridge's (36) paper. He analyzes the automatic protection which a large flock affords as follows: A single bird might be in error from (a) confusion with regard to proper direction of flight, (b) effect of heavy winds or thick fogs acting as a temporary confusing factor while migrating, (c) gradual deviation from the course due to unequal wing power. A large flock eliminates these causes of error to a large extent and the origin of the flock is probably due to the fact that it is protective. The errors are averaged by numbers. He does not attempt to explain the 'sense of direction' but rather the mechanism to avoid getting lost. Night migratory calls are discussed and the protective form of certain flight formations as *e. g.*, the echelon. This he thinks is not taken to prevent any interference but so that each bird may see both forward and to the side at the same time. Birds instinctively follow one another.

Some experiments in feeding humming birds which were continued for seven summers are described by Sherman (32).

The birds learned to sip sirup from bottles concealed in flower forms and from an unconcealed bottle fastened to a post. The amount of sugar taken by a bird per day amounted to 110 to 120 minims. From the behavior observations it seemed probable that the same birds were coming back summer after summer. They nested in a near by wood and only came to the garden for their food. All the birds which fed from the bottles were females.

Strong (33) studied the Herring Gulls both in their breeding places and in confinement at the University of Chicago and has written up his results in two papers and Tyler (38) has published some notes on the nest life of the Brown Creeper in Massachusetts.

LEARNING

Many of the studies in animal learning have been noticed under the head of the particular sense control which was involved. The articles are not so numerous as in 1913. Basset's (2) study of habit formation in a strain of rats of less than normal brain weight may be mentioned. The rats were some derived from experimental inbreeding at the Wistar Institute of Anatomy and Biology and their relative brain weight (relative to body length) was 6½ per cent. less than that of their normal controls. Basset worked with several generations of these for over two years on maze and puzzle box problems and he used in all 62 inbred animals and 62 normal controls. Tests were made not only for learning but also for retention and for relearning. In all of the experiments the rats of lesser brain weight did poorer work on an average than did the normal control series. The inbred rats of the seventh generation worked less well than those of the sixth and those of the eighth generation not so well as those of the seventh. "It would seem," the author says, "although lessening brain weight had ceased after the fourth generation that the ability to form habits lessened progressively with successive generations of inbreeding. The writer nowhere urges that this lesser ability is due to the inbreeding *per se*.

Of a different type and far less well controlled is some work which is reported in a paper on learning and relearning which comes from the Bedford College for Women, University of London (20). The work was under the immediate control of two students but was also used for class demonstration. As is clearly stated the work is a repetition of old problems and makes no pretence to originality. The straight and square mazes described by Yerkes in the Dancing Mouse were the mazes used and in addition a maze in five sections which was built for this experiment. There were only two mice used on this apparatus. Mouse 'M' learned the straight maze, then the same maze reversed, then the square maze, then this maze reversed and then relearned both of these mazes at varying intervals in their original positions. Mouse 'S' followed the same order, only it began with the square maze. Both learned the sectional maze, relearned it and learned it in reversed position. Many individual learning curves and much data are given. Three rats worked on two of Small's puzzle boxes. The general conclusions are as follows: (1) With renewed repetitions there is a steady advance in learning. This advance, however, bears no direct relation to the interval which elapses between one series of repetitions and another. (2) With sufficient repetition the successful response may become so " well known " as to be unaffected by the lapse of long intervals. (3) The successful response is developed in connection with the general meaning of the situation. The experiments do not warrant us in saying in what experiences this general meaning consists. Retention of it, however, is of even greater importance in influencing the progress of relearning on a subsequent occasion than retention of the series of successful movements. Thus the learning does not fall entirely under the law of habit. The successful movements acquired in learning one maze do not hinder the mice in learning another which demands a different series of turns; on the contrary, learning which takes place after practice on other work is more successful than relearning which follows on a period of idleness. * * * The influence of the general meaning of the situation is more marked in the learning of the puzzle boxes by the rats than in the learning of mazes by the mice."

The Elberfeld horses still continue to engage the attention of our friends across the water. Moekel (22) thinks the Mannheim dog's behavior much more spontaneous than that of the horses. Máday (21) attempts an elaborate analysis of the mental functions of man, their forms and the conditions which evoke them and then examines the proof of the same in the Elberfeld horses. Piéron (25) whose ideas on the subject are a little more sane than those of some other investigators gives an excellent review of the situation with a very complete bibliography.

A careful report by Haenel (7) shows that the horses seem to have lost ground during the year. Krall attributes this to the almost unbroken succession of visitors and their many questions. Krall admitted that the vocabulary of Zarif had decidedly suffered and that many of the arithmetical operations which Muhammed formerly performed were now beyond him. Barto, the blind horse, which had previously acquired the foundations of arithmetic in a few days, had, in the summer, after five weeks vacation, forgotten it all, and learned it the second time with much greater difficulty than at first and had not yet acquired his former facility. All of the first horses which Krall undertook to train were teachable—had understanding as he thought. But during 1914 he had three horses from the stud of the King of Wurtemberg and after three or four weeks endeavor he was forced to confess that he could teach them nothing and had to send them back as unlearned as they came. He has similarly failed with a young female elephant. Those who were inclined, with Krall, to rate the intelligence of the horse very high wonder now if these horses have reached the limit of their ability. Others ask, is it not possible that the criticism which has been made upon these claims has influenced Krall so that clues which the horses could use in their training, possibly unconscious to Krall himself, are being excluded.

One of the best articles on the subject is a critical paper by Schroeder (28) which not only inquires into the proof of the facts but also into the philosophical and scientific implications of the theories which have arisen in their explanation.

GENERAL PAPERS

Besides these articles reported above there are others of a more general nature which do not properly belong under any of the above headings. Hubbert (11) discusses the value of units of time versus units of distance in learning. In the way of apparatus four graphic methods of recording maze reactions are described by Yerkes and Kellogg (40) which they designate as (a) the direct method, (b) the simple reflection method, (c) the double reflection method of Watson and (d) the double reflection method of Kellogg. The authors discuss the advantages and disadvantages of each scheme and their paper is followed in the same journal by one by Watson (39) in which he describes his own recording device in greater detail. His circular maze is a maze so planned as to make it very easy to increase the complexity by adding new units, etc., and with this maze goes his excellent recording device.

A sympathetic description of the Pawlow method with animals is found in Morgulis' paper (24). The article is devoted chiefly to the neuro-psychical phases but the method with its implications is of interest to all who work with vertebrate animals.

Similarly the article by Carr (3a) on the Principles of Selection in Animal Learning will attract all who attempt to analyze animal behavior or to explain their mode of learning. It is impossible to summarize an article which is itself so compact. The author says, " Selection and elimination are the diverse effects of a single process or mechanism. All connections tend to be preserved; all develop in strength and functional efficiency during the learning process, but their development proceeds unequally. The unsuccessful tendencies are not eliminated in the sense of being torn out by the roots; they are eliminated only in the sense of not being aroused in that situation. The strongest and most prepotent tendencies of the group function first and dominate the situation. The successful act is selected because it finally becomes the most prepotent in the group; all others are eliminated, or better are 'suppressed' because of their lesser development in functional efficiency. . .

"The problem of determining the various principles of selection thus resolves itself into a search for those factors which favor the retentive development of the successful act at the expense of the many failures. These principles are relative recency, relative frequency, and relative intensity."

These principles are then applied in a careful analysis to three types of animal problems. The paper concludes with a brief comparison of these selective principles with that of pleasure-pain which has been advocated so frequently by others.

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WATSON'S "BEHAVIOR"[1]

E. L. THORNDIKE AND C. J. HERRICK

Professor Watson's "Behavior" is not only an admirable introduction to comparative psychology; it is also an important record of the methods and ideals of investigation approved by a leading investigator and a stimulating account of his views on the general problems of animal life and intelligence.

Examining it from the first point of view, one finds a clear and readable statement of representative problems, of apparatus and methods, of what is known and opined concerning instinctive tendencies, habit formation, imitation and other possible secondary sorts of learning, of the limits of educability, of the relation of human behavior to that of other animals, and of the sense-powers of animals. Every teacher of psychology who acknowledges the need of providing knowledge concerning animal psychology is in Watson's debt. With Washburn's book for the analysts, Watson's for the behaviorists, and both together for the ordinary matter-of-fact psychologist, the teaching of animal psychology should be notably efficient. It is interesting to note that animal psychology is now in a position to mete out to the anecdotal school the strongest form of denial—neglect. Watson, if I remember correctly, nowhere quotes or refers to Romanes or any of his like. This is probably wise, though pedagogically the contrast in question is one of the best beginnings for a student.

There are three topics which the reviewer at least wishes Watson might have included for the student's sake and one which might perhaps better have been left out. First, the behavior of the micro-organisms should, I think, have had a special chapter in addition to the incidental references made. Indeed some of these references are likely, as they stand, to be unintelligible to many students. Second, concrete cases of the phylogeny of behavior, such as Whitman's story of incubation or the course of the scratch reflex, with a discussion of the problems of tracing the growth and differentiation of behavior as a fact, seem to me among the most stimulating facts of animal psychology. The arguments concerning the causes of variation in general and the potency of sexual selection in general might well be omitted in favor of the more specific and more relevant concrete story of behavior's natural history in the world. In the third place, I regret the omission of a chapter concerning objective methods and results in *human* psychology. The student is likely, as Watson's book stands, to be left with the impression that mental chemistry—the analysis of conscious states into elements and the construction of cross-sections of a stream of consciousness out of sensations, affections, and other Wundtian myths—has been the regular, orthodox thing in human psychology. On the contrary, objective methods and results have characterized a very large proportion of the work of recognized psychologists for thirty years. Ebbinghaus' *Memory* and Cattell's studies of reaction-time, for example, are as 'behavioristic' or objective as Bassett's study of rats or Yerkes' study of frogs.

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[1] Behavior: An Introduction to Comparative Psychology. By John B. Watson, New York, 1914, xii+439 pp.

Watson has, throughout the book, freely joined to the description of the status of animal psychology a plea for rigorous control of conditions and steady aim at prophesy of behavior as a test of the truth of conclusions. One feels the zeal of the investigator for sound research and the faith of the scientific man in matter of fact control and prediction as the justification of science. There is also the healthy insistence that our eventual ideal must be an explanation of intellect, character and skill in terms of known neural mechanisms. All this, though perhaps somewhat over the heads of students, is healthy, and helps to make the book a truer picture of the status of animal psychology, whose workers have worked in comparative freedom from obscurantist conventions.

The third contribution of the book is the systematic expression of Watson's views of the folly of introspective analysis, the non-existence of centrally initiated processes, the relation of pleasure to afferent impulses from the erogenic zones, the adequacy of speech movements and other muscular responses to account for what is commonly meant by 'thought,' the structural unmodifiability of the neurones from soon after birth, and the adequacy of frequency and recency to explain all the dynamics of learning. These views will interest psychologists even though they know or care little about the details of animal activities. It is, of course, impossible to do justice to them either in description or evaluation within the limits of these pages. In the reviewer's opinion all of them are important, but also, with one exception, are too extreme to be correct as stated.

Watson seems to me to neglect the facts that a human being can observe himself not only as he observes another human being but also by other avenues, and that this information about oneself, got irrespective of sense-organs, may well play some part in science. It is a minor part, but not necessarily zero. That "there are no centrally initiated processes" seems flatly false at its face-value, and, even when interpreted by a conservative understanding of Watson's account of implicit behavior—that is, of the procedure occurring in very long-delayed reactions—seems to imply that all the hundreds of millions of secondary circuits of associative neurones are doomed to inactivity except when stimulated within a half-second or so by sensory neurones. Perhaps I have misunderstood his position on this point. The limitation of pleasure to stimuli from the sex zones seems dubious in view of the apparently closer attachment of pleasure to tastes and smells and its apparent lack of any such rise and fall as the sex-zone sensitivities show. The doctrine that the neurones stay the same structurally from birth, or soon thereafter, is, I am aware, fashionable, but it is speculative, and the opposite speculation—that the terminal arborizations and collaterals of the neurones grow here and dwindle there—seems to me more in accord with known facts of growth, degeneration and regeneration. Theories of behavior should not pin their faith to either theory.

The doctrine that the 'successful' response is selected and associated with the situation, not because of its success, but because it has been made as a response to that situation oftener than any other one response, seems substantially identical with the similar doctrine of Stevenson Smith. The argument holds, as I have shown in discussing Stevenson Smith's presentation of it, only if by original nature the 'successful' response has nearly as great a probability of occurrence as any other one. Watson, like Smith, neglects the common case of learning of the type:—

Situation	S,	Trial	1,	Day	1,	Responses	1, 2, 1, 1, 1, 2, 1, 1, 3, 1, 1, 4	(4 bringing food)
"	S,	"	2,	"	2,	"	1, 1, 1, 2, 2, 1, 2, 3, 5, 1, 4	(4 bringing food)
"	S,	"	3,	"	3,	"	1, 2, 1, 1, 6, 1, 3, 1, 4	(4 bringing food)
"	S,	"	4,	"	4,	"	1, 1, 2, 1, 4	(4 bringing food)
"	S,	"	5,	"	5,	"	1, 3, 1, 1, 4	(4 bringing food)
"	S,	"	6,	"	6,	"	1, 2, 1, 3, 4	(4 bringing food)
"	S,	"	7,	"	7,	"	1, 2, 1, 4	(4 bringing food)
"	S,	"	8,	"	8,	"	1, 4	(4 bringing food)
"	S,	"	9,	"	9,	"	4	(4 bringing food)
"	S,	"	10,	"	10,	"	4	(4 bringing food)

Here response 1 starts out with a frequency of 8 to 1 and yet loses in the end. Such cases are very common in learning.

I have registered these objections to Watson's views largely because it seems desirable to keep the general aims and methods of objective psychology distinct from the particular explanatory hypotheses of any one of us who are studying it.

In his emphasis on the prevalence of actual speech movements as the body, and perhaps even the soul, of thought, Watson seems to be following a much more hopeful hypothesis. Thought does seem to be in the beginning, as Cooley has said, "a species of conversation" and throughout life what many introspectionists call images of words are almost certainly often actual partial enunciations. The time-honored 'think bubble' experiment, for example, is not a test of the presence of kinesthetic *images*, but of actual movements—evidence of a kinesthetic image would be found rather if one *could* think of saying the word without moving the mouth-parts. Human behavior in thinking does consist of muscular responses, the sensations thereof, further responses excited thereby, and so on, to a much greater extent than the older "train of thought" metaphors suggested. A large residuum of thought that involves only intracerebral neurones does, in my opinion, exist, as witnessed in the mental manipulation of space relations in geometry, engineering, and the like, or sound relations in musical composition; but Watson has exposed a weak spot in psychology's neglect of the actual muscular action that goes on in thought and confusion of it and sensations due to it with kinesthetic *images*.

A reviewer of this book is presumably expected to make some estimate of Watson's contrast of the general merits of the study of consciousness and the study of behavior, as means to the progress of science. Watson seems to me to offer the right criterion in *power of prophecy.* The proper criticism of the analysis of conscious states and synthesis of supposed conscious elements at which gifted followers of Wundt have busied themselves for a generation seems to be that these labors have so seldom enabled us to prophecy what any animal, human or other, would actually think or feel or do in even a dozen situations. Where we do find power of prophecy attained, we commonly find that objective study of what the subjects of the experiments have said or done has given it. The trouble seems to be, not that pure psychics, or the inner life of a man as he feels it, does not exist and give facts, but that it gives facts to only one observer, and that, first, we get on much better by using his *testimony* about these facts (which is, of course, his behavior, verbal or otherwise) by the ordinary methods of science than we do by leaving him to try to draw inferences from it in some more direct way. In fact, he himself does as well or better by reporting the inspections of himself which he makes without using his sense organs to himself by inner speech and the like and using them thereafter as he would use the reports of any other man. In the second place, these one-man, unverifiable observations do not work as well in science as observations made *via* sense organs which many of us can make together and which we can repeat.

Watson is probably right, also, in asserting that straightforward objective work has been more or less hampered by the fashion in psychology of attempting always to say something about some purely psychical fact. The protocols on the conscious accompaniments of reaction-time experiments, discriminations of sensory differences, and measurements of 'thresholds' of intensity, for example, it might be torture for Watson to write, collect or read; and if his book relieves future Watsons from being conscience-smitten at not contributing to knowledge of how a frog feels to himself when he croaks or what the stream of a rat's consciousness is as he scampers through the maze, it will serve thereby a worthy end.

In any case the spirit of psychology in America seems now to be in a healthy condition in encouraging individuals each to do the work he thinks best in the way that he thinks best, and in judging work by the truth and usefulness of its results rather than by the orthodoxy of its presuppositions or methods. For students of the subjective side of the world by personal inspection of one's own inner life to regard their work as that of a psychological élite, pure-breds, untainted by physiology, sociology, psychiatry or education, would now be amusing rather than objectionable. For students of objective behavior to regard themselves as martyrs, heroes or prophets is now unnecessary.

E. L. Thorndike.

The writer has been asked to add some comments from the biological standpoint to Professor Thorndike's review of Watson's Behavior. It is a pleasure to do this, for Doctor Watson's biological training, wide reading and accurate scholarship are everywhere reflected in this work. There are only a few additional points where comment from the biological side suggests itself to me.

The first point is a very minor one, which suggests, however, some reflections of wider import. In commenting upon the backward condition of the anatomy and physiology of the nervous system, a number of interesting problems are suggested, such as the nature of nervous impulses, the processes which make for the adaptation of sense organs and the like. Then follows the rather disquieting statement, "In this day of advanced physiological and neurological technique surely the only difficulty in obtaining satisfactory answers to these questions is the lack of sufficient interest on the part of the men who are competent to carry out such researches."

The fact is that the number of researches directed toward such neurological problems is fairly large—far greater than one man who devotes his whole time to neurological work can master if he attempts any original work himself. Must we then infer that the fundamental difficulty is that so few of these numerous workers are really "competent to carry out such researches"? Possibly; but the real explanation for the relative sterility of so much of this arduous labor lies in the fact that the "advanced physiological and neurological technique" of today is wholly inadequate to open up most of the problems mentioned. "If the iron be blunt, and he do not whet the edge, then must he put to more strength." We need to whet the edge of our neurological endeavors by the acquisition of new points of view. The study of familiar facts in a new setting is often all that is necessary to point the way to entirely new methods of attack.

A review of neurological literature, especially in the field of comparative neurology, reveals a prodigious amount of research from which surprisingly few generalizations can be deduced which are of great interest to students of either animal behavior or human psychology. This literature has its own problems, in the solution of which it has not been wholly unsuccessful; but these problems have always been sharply circumscribed by the limitations of technique, not the least of which has been the failure of investigators in this field to make a correlated study of both the structure and the functions of their objects of research. Doctor Watson's recommendation that extensive programs of research be carried out with the cooperation of behaviorists, experimental physiologists and neurologists is a suggestion of constructive value. In short, while the technique of each discipline needs improvement, the greatest need is for a technique of cooperation.

In the discussion of instinct, biologists, behaviorists and psychologists all claim an interest. All behavior is complex, and it has been common for each student of animal life to select from this complex the particular factors which seemed best to fit into his own philosophical preconceptions and to use these factors only in formulating his conception of instinct.

In contrasting instinct and habit (p. 185) Doctor Watson clearly states the cardinal principle which alone can bring order out of the chaotic and hazy notions which are current. This principle is the sharp distinction between the innate and the acquired factors in behavior. All agree that a reflex is the function of an innate mechanism. Now when reflexes are combined, as we always find them in behavior complexes, the order and pattern of their combination may likewise be determined by the hereditary organization, or this pattern may be acquired during the individual life of the animal. In the former case we are dealing with a pure instinct; in the latter case with a pure habit. This is Watson's terminology. I would add, that, in any concrete example of behavior in a higher animal, both of these types are almost certain to be present, and so the particular act cannot as a rule be classified off-hand as instinctive or habitual. The best that we can hope to do is to analyze the act into its elements and then determine which factors are innate and which are acquired.

It has been my conviction for several years that the term instinct has outlived its usefulness in science. All behavior of organisms can be classed under two heads. It is either the function of an innate mechanism and therefore determined by the hereditary organization (reflexes, 'instincts'), or else it exhibits new combinations of elements whose pattern has been individually acquired. Habit is only a terminal phase of this individual modifiability. Both innate and individually variable action are found in some measure in all organisms, and, as stated above, in almost every act of the higher animals; and a more detailed consideration of the relations of these two factors at the beginning of the discussion of instinct might profitably replace some of the discussion of moot questions of general evolutionary theory in Chapter V.

There is a third topic in Doctor Watson's book about which it may be presumptuous for a mere biologist to express an opinion, though it most assuredly has a biological aspect. The new school of experimentalists has sought to rescue the study of animal behavior from the slough of anectodage and uncritical anthropomorphism into which it had fallen and to establish it on the secure scientific basis of objective and verifiable observation. In this their labors have already been crowned with a gratifying measure of success, and the future promises still greater gains. In such a book as this one, the author, accordingly, does well to adhere strictly to the program which has been so abundantly justified by results and to limit his discussions to what is objectively verifiable, leaving quite out of account, observations and speculations about possible mental processes of men or other animals. This is a sound scientific procedure.

But when he goes further and says that because the phenomena of consciousness as introspectively experienced are irrelevant to his special program, therefore they are everywhere else irrelevant and negligible, he seems to have thrown out the babe with the bath, and the biologist should be the first to protest. The new psychology may perhaps be able to dispense with consciousness, but biology cannot do so.

One hesitates to utter his convictions on the last point, for he is certain to be misunderstood. But conscious processes are realities which cannot be ignored in a comprehensive scheme of things. They are, moreover, positive biological factors in human evolution; and the biologist can see no reason why they should not be observed in the only way open to him, namely, by introspection.

Is there not, therefore, abundant justification for including consciousness, as introspectively known, as one of the elements of human behavior (and inferentially of the behavior of some other animals also), and should not any comprehensive scheme of behavior studies include this factor for what it is worth? The fact that in the past the uncritical use of these data and of hypotheses based thereon has often led us astray is no justification for denying their validity and practical value when properly used. Whether in any given program of research it is expedient to use these data, is a quite different question, which must be decided on its own merits in each case.

C. J. Herrick.

DUNLAP'S "AN OUTLINE OF PSYCHOBIOLOGY"[1]

C. JUDSON HERRICK

At the present time there is an active demand for a brief untechnical introduction to the structure and functions of the nervous system adapted for the use of students of psychology and education who have no biological training. It is the purpose of this little book on Psychobiology to fill this need. Every experienced teacher will recognize that the difficulties in the way of such an enterprise are almost insuperable, and any intelligently directed effort in this field should, accordingly, be judged leniently.

There are nine chapters in Dr. Dunlap's work, of which one is devoted to the cell, one to a survey of the tissues of the adult human body, one to muscular tissue, one to glandular tissue, and the remainder to the nervous system.

The discussion of the cell and tissues is in general correctly stated, but is rather schematic and lacking in functional coloring. The critical student will note a number of minor errors, not all of which can be explained as due to the condensed form necessary in a work of this sort. A few examples are given.

On page 11 we read, "Every plant and every animal commences its individual life as a single cell." Exception should, of course, be made of the very large numbers of species which may be propagated by fission and budding. On page 26: "Vascular tissue; This includes the blood and lymph, the lymph glands and the red marrow of the bones, and develops from the endoderm." The endodermal origin of the blood cells is controverted, but there is no controversy regarding the mesodermal origin of some of the other tissues here mentioned. On page 29 the nuclei of pale striated muscle fibers are said to be marginal, while those of dark striated muscle fibers are located more centrally, embedded among the fibrils. The converse is true, as illustrated by the figure of human muscle printed on the same page. On page 41 the cochlea and possibly the sensory surfaces of the organs of smell and taste are said to migrate outward from the anterior part of the medullary tube. On page 58, line 2 from the bottom, for "spinal" ganglion of the cochlea, read *spiral* ganglion.

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[1] Knight Dunlap. An Outline of Psychobiology. Baltimore, The Johns Hopkins Press, 1914, 121 pages, price $1.25.

The chapters on the nervous tissues comprise a total of 62 pages. There are numerous good figures illustrating the nervous system and its elements, accompanied by a running description and lists of names of the parts figured; but here again there is a dearth of functional interpretation, and the reader who attempts to assimilate this description with no previous preparation in biology may find it rather indigestible. The neurological chapters contain a few infelicitous expressions, such as the following:

There are several places in the descriptions of the nerves where the terms afferent and efferent are confused, some of these apparently being misprints. On page 82 we read, "Only four of the cranial nerves are, like the spinal, 'mixed nerves.' Of the other eight, three are pure afferent, or 'sensory,' and five are efferent, or 'motor'." But in the enumeration which follows the I, II and VIII pairs are described as afferent and the III, IV, V. VI, VII, and IX pairs are correctly described as mixed. There is no mention here of afferent fibers in the vagus and the "afferent axons of the spinal accessory" are said to supply the sterno-mastoid and trapezius muscles. On page 90, however, the afferent fibers of the vagus are referred to. On the page last mentioned the account of visceral fibers of the VII and X nerves is incorrect. The recent studies of Molhant, Yagita, Kosaka and others have clarified the relations of these systems.

The final chapter on the Functional Interrelation of Receptors, Neurons and Effectors is a very successful application of the author's cardinal principle (as stated in the Preface), that the body must be considered as a functional unit, and that this is even more important for psychology than for physiology. It is to be regretted that this principle, which is so well stated in general terms in this chapter, was not applied more explicitly and concretely in the descriptive part of the work. This brief final chapter alone is worth the price of the book.

HACHET-SOUPLET'S "DE L'ANIMAL A L'ENFANT"[1]

WALTER S. HUNTER
The University of Texas

In the present volume the author attempts to set forth the main outlines of an embryo science, that of comparative education. This discipline is to be based upon an experimental study of animals and children as opposed to the hitherto current *a priori* ideas. The child presents only a difference in degree and not in nature with respect to the higher animals. Hence methods of training suitable to the latter are applicable to the former. And Hachet-Souplet has had long experience in the training of animals.

In the elaboration of this program, the author treats first, animals and then children. His standard, however, is the child and animal life is interpreted in terms of this. The chapter headings and some of the topics of the first part are as follows: (1). Experimental study of sensations in animals. In addition to comments of a general nature, tests on pitch and visual intensity discrimination are described. Pigeons tested on the last problem gave results verifying Fechner's law—"du moins grossièrment," writes Hachet-Souplet. And one may well accept the qualification, for the most elementary precautions are ignored in the work which is hardly so scientific as the tests Galton once made with his whistle. (2). Fundamental and derived instincts. Hunger and fear (primitively the rejection of food) are fundamental and unmodifiable. Derived instincts are habits. It is interesting to note that the author does not follow Bergson in relating instinct and intelligence as opposites. (3) and (4). The experimental study of derived instincts. (5). The principal laws of the association of sensations. The chief law here is that of recurrence. Stimuli *d c b a* precede the reaction *r*. The associations formed remount from *a* to *d*. This law makes anticipation possible in that *d* or *c* can lead to *r* before *b* or *a* appears. At the close of this chapter the author announces that "des expériences, poursuivies pendant dix ans à *l'Institut de psychologie zoologique*, ont permis d'établir que les habitudes imposées aux animaux sont transmissibles par héredité." The following six chapters deal with intellectual activity, the notion of causality, the notion of the physical me, abstraction, aesthetic taste and persuasion as a method of education. The discussion of causality centers on the use of tools and the devising of novel methods of securing results. Hachet-Souplet claims to have established the existence of these types of activity in animals. The presence of an idea of the physical me in phyla below man is asserted on the basis of two tests: (I) A dog that responds readily to the command "come" when alone will refuse to do so if he sits in company with other dogs. He will respond, however, when his *name* is called. He *knows* that there are other dogs present. (2) Monkeys will amuse themselves and grimace before a mirror. Critical comment is unnecessary here.

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[1] Hachet-Souplet, P. De l'Animal à l'Enfant. Pp. 176. Paris, Alcan. 1913.

The second part of the book dealing with children includes four chapters: animal traits in children, punishment and reward, moral training, and instruction properly speaking. A firm discipline, a sort of "dressage" should be applied in early infancy in the light of the "law of recurrence."

It is unfortunate that rigorous scientific methods could not be applied where the opportunities are so great as they seem to be at the *Institut de psychologie zoologique*. In the reviewer's opinion attempts to combine behavior work and education in any intimate manner are at present far-fetched and are likely to result in the deterioration of the scientific character of the work.

KAFKA'S "EINFUHRUNG IN DIE TIERPSYCHOLOGIE"[1]

WALTER S. HUNTER
The University of Texas

If not an encyclopedia, Kafka's work is at least a very extensive compendium of the literature on invertebrate behavior. His sources are almost entirely experimental. A second volume is promised which will consider the senses of vertebrates and the development of the higher psychic capacities in the animal kingdom (instinct, memory, intelligence, etc.). It is that volume to which students will turn for Kafka's theoretical discussions which are to be based upon a wide survey of experimental facts. It is to be hoped that the war in Europe will not cause undue delay in this scientific enterprise, for the spirit of the first volume leads one to expect a sane handling of the literature and problems of the higher functional activities in the second volume.

The present work emphasizes the sensory processes of invertebrates, and the material is organized upon this basis. Touch (117 pages), the static sense (41 pages), hearing (33 pages), the temperature sense (15 pages), the chemical sense (97 pages), vision (156 pages), the space sense (53 pages) and the time sense (20 pages) are the chapter headings and the amounts of space allotted to each topic. In each of the above divisions the data are grouped according to animal phyla and genera—protozoa, coelenterates, worms, mollusks and arthropodes. This method of presentation has its defects as well as its advantages. It offers readier reference advantages to the zoologist who is interested in phyla than it does to the behaviorist who is interested in activities. The scheme allows no place for a summary of data bearing, e. g., upon tropisms and of the various theories thereof. Nor does it open the way to a statement of the essential facts of nervous integration. Such facts as are just indicated are to be found scattered throughout a large volume, if they are found at all. In some cases they are not even brought to the reader's attention in the subject index. One such case is that of tactile and auditory hairs. A volume placing emphasis upon sensory discrimination should contain a discussion or a summary of methods with evaluating comments thereon. This would give the reader a guide to the reliability of the data presented. An actual examination reveals that the method of general response is very universally applied. This can be supplemented by extirpation studies with certain forms. The association method in any definite form has not been so extensively used with the invertebrates. Further criticism in this same vein may be directed against the author's bibliography. Although this does not aim at completeness, it does aim at the inclusion of the most important works and of the most representative general references. The list given covers 28 pages. In the general list one misses such titles as: Wheeler "Ants"; Holmes "Evolution of Animal Intelligence"; and Max Meyer's "Laws of Human Behavior,"—which last deserves a place equally with many that are included. Other sins of omission might be indicated here and in the bibliographies in special subjects. I will call attention only to the lack of reference to McIndoo's "Lyriform Organs and Tactile Hairs of Araneads" and to the articles on taste and smell mentioned below. American investigations are referred to with great frequency.

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[1] Kafka, Gustav. Einführung in die Tierpsychologie. Bd. 1, S. xii+593. Leipzig, Barth, 1914.

One of the very excellent features of Kafka's work is the large number of illustrations given,—a total of 362 figures. These present the gross bodily appearance, the detailed anatomy of the sense organs and certain types of reactions of the animals concerned. Comparative psychologists will welcome a convenient summary of invertebrate sense organs. The teaching of the subject will gain by this emphasis upon structure even if research work does not need correction and an added incentive. Pedagogically important also are the references to the tropic activities of bacteria and the sex cells.

The intimate relations between touch and hearing are pointed out. The existence of auditory hairs furnishes supporting evidence here. No claim is made that the phenomena treated under audition are to be interpreted as essentially different from those of touch. The differentiation must be in terms of stimuli and these are proverbially hard to control. The general problem of the criteria for distinguishing sense fields is not taken up. A special case that is considered is the relation of taste and smell. The differentiation here is on the basis of the topographical relations of the sense organs which lead to functional differences. Taste functions in the immediate taking of food while smell leads to the search for food. [In other words, it is a difference of extero and intero-ceptors.] If the question of the relation of taste and smell was to be handled at all, the author should by no means have ignored the work on this topic by C. J. Herrick, Parker and Parker and Stabler. The antennae of insects are held to contain the olfactory organs. Miss Fielde's work on the detailed anatomy of the antennae of ants is quoted with approval (?). In his discussion of the study of smell by extirpation methods, Kafka makes the very valuable suggestion that the presence of inadequate but effective stimuli must be reckoned with both in so far as they may affect cutaneous nerves and in so far as they may arouse activities in taste. The presence of this source of error is very probable where intense stimuli are used [and really can only be thoroughly guarded against when data on threshold sensitivities are at hand.]

In discussing the factors that cause insects to seek flowers, the author opposes Plateau's odor theory and supports Forel and others who find vision and habit the main factors. The recent work of Lovell furnishes additional confirmation of the truth of this point of view.

The treatment of vision is well executed. Phototropisms are discussed in detail and are interpreted from the standpoint of "trial and error" rather than from Loeb's point of view. The author points out (page 321) "dass die 'Richtung der Lichtstrahlen' an sich ein ebenso 'metaphysisches' Erklärungsprincip darstellt wie etwa die ' Willenstätigkeit,' gegen deren Heranziehung gerade die Vertreter der Tropismenlehre so energischen Einspruch erheben. Denn wie die Einstellung eines Organismus durch die *Richtung* bestimmt werden soll, in *der die Lichtstrahlen seine Körpersubstance durchsetzen* (Sachs), bleibt rätselhaft * * *." This chapter contains a large number of diagrams illustrating the discussion of the evolution of the invertebrate eye. In the presentation of the much be-labored field of insect color-vision, the author leans toward the interpretation that the behavior in question is guided by brightness only. Kafka rarely introduces theoretical psychological comments and discussions into the text. It is a pleasure, however, here in the account of color-vision, to meet the following statement: "Die Vergleichende Psychologie vermag daher auf objectiven Wege das Problem des Farbensinnes der Insekten nur bis zu der Feststellung zu fördern, dass jedenfalls verschiedene Strahlengattungen verschiedene objective Wirkungen hervorbringen, sie kann dagegen auf die subjektiven Phänomene im tierischen Bewusstsein wiederum nur aus der Analogie der objektiven Vorgänge im menschlichen und im tierischen Organismus schliessen." (S. 473). Such analogies, the author further points out, are hindered by our lack of information concerning retinal processes in man.

American readers will note with interest Kafka's Introduction dealing with the aims and principles of comparative psychology.[1] It was written prior to the publication in this country of the many recent "Behavior" papers. In the present book, the discussion covers 16 pages only out of a total of 549. The "speculative tendency" thus plays no overshadowing rôle here.

The fact that comparative psychology has manifested an anthropomorphic tendency cannot be used as a final argument against the possibility of its being thoroughly scientific. Biology, physics and chemistry have passed through similar stages. Yet reacting against anthropomorphism, natural science tends to seek all explanation in physical and chemical terms or at most in the teleological conditioning of reactions. Conscious processes, since they cannot be "observed" are ruled out of the subject matter. There is no doubt that the rapid progress made by comparative physiology and biology within recent years has been largely due to the insistence that objective processes be stated in terms of objective factors. Appeals to psychic processes have most often indicated only the faliure to analyze properly a causal nexus. Yet that this physico-chemical statement is not exhaustive is testified to by the consciousness that each one has of mental concomitants of bodily activity as well as by the appeal to introspection which physiology makes in its studies of brain functions. The inaccessibility to immediate experience of the facts for which comparative psychology seeks is no more a criticism of the scientific character of the field than is the impossibility of an immediate experience of the center of the earth and of the back side of the moon an objection to the sciences there concerned.

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[1] This chapter appeared, prior to the publication of the book, under the title "Ueber Grundlagen und Ziele einer wissenschaftlichen Tierpsychologie," *Arch. f. d. ges. Psych.*, Bd. 29, 1913.

There is as much justification for attributing consciousness to animals as to one's fellow men. In neither case can immediate experience be had. It is useless to seek for objective criteria of consciousness, although one feels impelled to do so. One of the most important criteria proposed is the "associative memory" of Loeb and Bethe. This, however, assumes that memory is the first thing in the way of consciousness,—a theory which can be traced as far back as Hobbes and Locke. [K. does not mention the fact, but this criticism has been urged by other writers, S. J. Holmes, *et. al.*] Other criteria based upon the analogies of human and animal sense organs and nervous systems ignore the possibility of the existence of consciousnesses different from the human. The legitimate use of analogy directs attention to the similarity throughout the animal kingdom with respect to biological adjustments: self-preservation, continuation of the species, avoidance of pain and the seeking of pleasure. The continuity of life on the physical side suggests a similar continuity on the mental side. The psychologist's task is to trace origins and growths within the subjective realm which it is necessary to posit beside the physical world. From the uncertainty which attaches to any subject matter not open to immediate experience, comparative psychology derives only the "Verpflichtung, sich streng an die Ergebnisse der objektiven Forschung als ihre einzige Grundlage zu halten, ohne sich dazu verleiten zu lassen, psychologische Interpretationen als kausale Erklärungen der physischen Phänomene auszugeben." (S. 13).

The reviewer can heartily commend Kafka's general point of view. A safe middle ground is held with respect to a question where extreme doctrines are only too frequently current.

CESARESCO'S PSYCHOLOGY AND TRAINING OF THE HORSE[1]

Carl Rahn
University of Illinois

This work represents the product of life-long study and practice of the art of horsemanship. The first volume deals with the mental and physical nature of the horse; the remaining three volumes are devoted to the methods of training.

We are told that " the horse's intelligence is limited, but the animal is intelligent enough to understand that it must have regard for what happens in its environment, and for its rider,—to feel the justice or injustice of the punishments inflicted,—to try to oppose, anticipate, and neutralize the efforts of its rider,—and to choose that moment for injuring its master when the latter is not looking." " The horse," furthermore, " has a highly developed imagination," and this combined with its great susceptibility to fear, makes the animal readily amenable to our attempts to train it, for the one "enables it to grasp readily the idea of our superiority" and the other "gives value to the slightest stimulus or chastisement."

In whatever way we may react to these psychological interpretations, the account of the methods of training will possess a positive value for the student of animal behavior, *viz.:* as a stimulus toward the formulation of specific problems in connection with the behavior of the horse. Such a student will miss with regret, at times, the minute analyses of stimulus and the significant classifications and tabulations of responses which, *e. g.*, make the work of Pfungst so valuable. Thus the changes in the voice of the rider are emphasized as one of the most effective helps, or means of control, and it would have been highly desirable to have had an analysis of the kinds of qualitative or other changes in the form of stimulation that constitute the really effective factor. At other points in the treatise such a criticism will not, of course, hold: for instance in the description of gestures and caresses as inducing stimuli. In the main, however, the reader is left with a feeling that the description is couched too often in subjective terms such as: "approval," "disapproval," "menacing." . . . . But the student will find suggested, on the other hand, a wealth of problems peculiarly significant. And their significance lies in this: that the horse presents, probably more favorably than any other form, opportunities for studying a mechanism of stimulus and response approximating very closely the typical social situation.

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[1] Cesaresco, Count Eugenio Mantinengo: L'Arte Di Cavalcare, Con Aggiunto: Il Cavallo Attaccato Alla Carrozza. Devoti, Salò, 1914.

If Pfungst's work establishes the fact of a fine perception of minimal movements of all kinds, as involved in the functionally effective stimulus for responses in the horse, the treatise of Cesaresco indicates what appears to be the salient characteristic of the responsive phase, *viz.*: a finely balanced inhibition-mechanism. It is this fact, plus the sensitiveness of the horse to minimal changes in stimulation while the response is in progress, that makes its behavior so closely analogous to the reaction of the human individual in the social situation.

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